NATURAL SCIENCE

A Monthly Review of Scientific Progress

No. 80—Vol. XIII—OCTOBER 1898

NOTES AND COMMENTS

Wanted, an Editor!

It is with the deepest regret that we find ourselves compelled to announce a step which may, we fear, result in the cessation of this Review at the close of the present year. For a time that, as the history of journals goes, is short, but that, when taken from the life of individuals in its most active period, appears long indeed, we have endeavoured to maintain the fortunes of *Natural Science*. The labour that this has entailed has undoubtedly been one of love; but, as years advance, responsibilities increase, and the time at the disposal of those who conduct this Review becomes less. We have therefore decided to discontinue the editing of *Natural Science* after the next December number.

It may be imagined that we do not take this step without anxious deliberation. We believe ourselves, and we are told by others, that *Natural Science* has filled a place in scientific journalism occupied by no other periodical, at least in this country. We have endeavoured to be independent, an aim that it is often difficult to accomplish. We have sought to praise the good because it was good, and to censure the bad because it was bad. And if censure has sometimes seemed to overbalance praise, let the truism be remembered that there is far more bad than good in the world. We have endeavoured to be impartial and to bow to no authority save justice and reason; but we have also tried to recognise that our ideas of justice and reason might not always be those of other people. Hence we have allowed a free field to the champions of views unorthodox as well as orthodox. The reproach of omniscience and infallibility has, it is true, been laid to our charge. But these sins of a private individual are the virtues of an editor. They may be accounted for by the fact that we have had freely placed at our disposal the pens of many, if not most, of the eminent biologists and geologists of the day, without distinction of nationality. To all our contributors, those whose names have been published, and those who have helped in the less grateful task of furnishing unsigned comments, we tender our heartfelt thanks.

We have said this much in praise of *Natural Science*, because we believe that a journal of this nature should not be allowed to drop altogether. We are prepared to hand it over, with all stock, appurtenances, and goodwill, to any scientific man who is prepared

to relieve us of all responsibility and to continue it as an independent journal. We had rather see it continue in other hands than drop out of existence altogether, and we believe that our opinion will be shared by most of our readers. Now is the time for expressions of admiration and sympathy to be translated into practical aid.

The Biological Exhibition at Bristol

Among the features of the British Association meeting that held out promise of novelty and interest was the widely-advertised Biological Exhibition arranged at the Zoological Gardens, Clifton. At 3 P.M. on September 8th, the entrance to the grounds was crowded by those who came to hear and see Sir John Lubbock open the exhibition. These numbers can hardly have been expected by those responsible for the arrangements, and it resulted that Sir John was seen by few and heard by fewer. The exhibition itself was attractive in many ways, and, considering the intense heat of these few days, remarkable as a *tour de force*; but to the biologist it was rather disappointing. The greater part of it was a flower-show, to which many leading firms of florists contributed. In this section the exhibit of most scientific interest was that of Dr E. J. Lowe, who showed beautiful examples of rare ferns, with crossed varieties of double, triple, and quadruple parentage. The zoological section contained a somewhat miscellaneous lot of exhibits, and it was clear that the committee had been prevented by the usual considerations from exercising that stern selection which alone could have maintained the desired standard. We, who are not thus hampered, need only mention the following:—Dr J. A. Norton of Bristol showed clutches containing cuckoos' eggs, in illustration of the various foster-parents, also a series of robins' nests with cuckoos' eggs, intended to elucidate the problem whether there be any variation in the egg according to the nest in which it is laid; Mr C. K. Rudge of Clifton, various nests of British birds, with an analysis of the materials from which they were formed. Mr G. C. Griffiths of Clifton had an interesting little case containing hybrids of Lepidoptera, examples of mimicry of plants by Lepidoptera (*e.g. Kallina inachis*, the Indian leaf-butterfly and its allies), and instances of protective and aggressive resemblances among Orthoptera. Mr H. A. Francis of Clifton showed specimens of local wasps and wild bees, with examples of protective mimicry of the same by other insects. Professor E. B. Poulton exhibited the proof quite recently obtained by Mr Guy A. K. Marshall, that *Precis octavia-natalensis* and *P. sesamus* are but seasonal variations of the same species: the parent specimen, here exhibited, of the form *P. octavia-natalensis* laid three eggs on February 27, 1898; on April 15, one of these produced a *P.*

sesamus, here exhibited; and on April 20, another egg produced *P. octavia-natalensis*, also shown. Mr F. G. Richmond of Braunton, Devon, had arranged some aquaria containing various species and hybrids of trout and salmon. The exhibit of the Marine Biological Association was of interest, not so much for the species shown as for the illustration it afforded of success, under the most difficult conditions, of keeping marine animals of various kinds alive by the constant circulation of the water. There were five shallow wooden tanks and about 400 gallons of sea-water was used. The water entered the tanks from a small upper reservoir by means of glass siphons, and after passing through them collected in two lower reservoirs, from which it was periodically pumped into the upper one again. The pump used was an ordinary semi-rotary yacht pump. Most of the fish, as well as many of the invertebrats, had been living in a healthy condition in the tanks for some two weeks previous to the opening of the exhibition, and there had been comparatively few deaths. Naturalists were also glad to have the opportunity of seeing in operation the ingenious plungers devised by Mr E. T. Browne and Dr E. J. Allen, and worked automatically by means of the fresh-water supply filling a can that was intermittently emptied by a siphon. Such a plunger keeps the sea-water in a bell-jar aquarium in constant movement, and serves to keep medusae alive and to rear the larvae of marine animals to the adult stage.

Bread out of Air

Sir William Crookes in his presidential address to the British Association traversed a wide field, practical, physical and psychical, in each department applying or suggesting the application of the most recent advances in modern physical research.

Starting with the question of late so much mooted, of the wheat-supply of these islands, intimately connected with the wider question as to the possible insufficiency of the wheat-supply of the whole world at no distant date, Sir William showed that whereas the consumption of wheat was increasing, the amount of land available for its production was strictly limited, and that already those limits were nearly reached. It is therefore necessary to increase the fertility of the land by means of nitrogenous manures. Unfortunately that special sanitary appliance which is the glory of our country, and which she has presented to all parts of the civilised world, has the disadvantage of wasting, in this country alone, "fixed nitrogen to the value of no less than £16,000,000 per annum." But even if we relinquish the system that Liebig stigmatised as "a sinful violation of the divine laws of nature," we shall not repair the mischief already done. Already the world's

deposits of guano are becoming exhausted, and our final resource, the nitrate-fields of Chile, cannot last more than fifty years, even if the demands upon them be not increased. The solution proposed by Sir William Crookes is the formation of nitrate by the combustion of the atmosphere. This can be effected, he says, by passing a strong induction current between terminals. By utilising natural sources of power, an electric current may be obtained that will enable nitrate of soda to be produced at less than £5 per ton, two-thirds the price of Chile nitrate.

The Mysteries of Matter

The second portion of the Presidential address was interesting chiefly for its speculations on the constitution of matter. Here Sir William adduced the various recent discoveries confirmatory of his own views, so long opposed, as to the existence of molecular streams of electrified radiant matter. The task of the future is to render available the energy contained in matter that to outward appearance is quiescent. The phosphorescence of uranium, and in a higher degree of the newly discovered polonium, is due to its power of extracting such energy, however that power be explicable. A new application of the principles that underly his theory of radiant matter has within the last few weeks enabled Sir William Crookes to add another to his remarkable successes in the fractionation and spectrographic study of the rare earths. He believes that he has demonstrated the existence of yet another element, which he terms Monium, because the lines of its spectrum stand alone, almost at the extreme end of the ultra-violet.

These far-reaching speculations as to the existence of energy, barely thinkable and yet capable of investigation, of measurement, and of utilisation, naturally led Sir William Crookes on to the most debateable and most confessedly speculative part of his address. To his previously published statements on the subject of psychical research, he adheres. However far we may accompany or lag behind Sir William in acceptance of the alleged phenomena of telepathy, this at least we must recognise in his words: the belief on the one hand that the enquiry has not yet reached the scientific stage of certainty; on the other hand, that any explanation will be an extension of theories of the constitution of the material universe already widely held and serving as the basis of actual experiment. Whether the suggestion, which we imagine to be implied, that telepathy is analogous to wireless telegraphy be accepted by physiologists matters little. We believe that it is right of Sir William Crookes to allude to these matters from the President's chair, since we think that for their investigation is demanded the co-operation of the keenest intellects in all branches of science. "To ignore the

subject would be"—not only for Sir William Crookes—"an act of cowardice."

Stereochemistry and Vitalism

In connection with the above remarks, it is interesting to note that the Presidents of the Sections for Mathematical and Physical Science and Chemistry—Professors Ayrton and Japp—dealt with subjects which might equally well have been considered in the Biological Sections, while the President of the Zoological Section applied to certain biological problems the methods of mathematics. Professor Ayrton described certain interesting experiments on the smells of substances. Professor Japp considered certain facts of stereochemistry in their relation to the fundamental problem of life. He pointed out that all inorganic compounds were symmetric, and that the forces producing them were either symmetric, or, if asymmetric, then asymmetric in two opposite senses. Compounds of one-sided asymmetry originate with the living world, and are only known to be produced by some selection in which living organisms must directly or indirectly take part. It is of course possible that some day the isolation of asymmetric compounds may be proved possible without the intervention of even the directing intelligence of the chemist in his laboratory. Professor Armstrong, who spoke after the address, evidently thought that this would be proved before the next meeting of the British Association in Bristol. We are as yet only on the threshold of the problem, and fresh methods of investigation or fresh conceptions may upset the prophecies of to-day. For the present, however, Professor Japp has done good service in setting before us one of the difficulties to be overcome before the vitalistic hypothesis can be rejected.

The Need of Numerical Investigation in Biology

Professor Weldon, choosing a subject which the ordinary naturalist is apt to consider abstruse and uninviting, succeeded in delivering an address that both for content and exposition was one of the successes of the meeting. Entitled, "Some objections to the theory of Natural Selection," it was in the main an attempt to expound to biologists the modern doctrine of chance, and to show that the variations which actually occur in the animal world are neither more nor less definite than those which result in the tossing of ha'pence or the casting of dice; further, that these 'chance variations' do afford scope for the action of Natural Selection in a way that admits of accurate measurement. The instance taken was the variation in frontal breadth of *Carcinus maenas* from a particular patch of beach in Plymouth between 1893 and 1898. It was shown that the frontal breadth was diminishing at a rapid rate in this particular race, and evidence was adduced to prove that this

was due to the selective action of an increase of silt in the water. Professor Weldon urged the necessity of extending as widely as possible this kind of numerical study. The difficulty of the theory of Natural Selection lies in the postulate that in any given case a small deviation from the mean character will be sufficiently useful or sufficiently harmful to affect the race. But determination of the deviation and of its effect on the death-rate is often possible. Whenever possible it is our duty to make it. "Numerical knowledge of this kind is the only ultimate test of the theory of Natural Selection, or of any other theory of any natural process whatever."

MORPHOLOGY

PROFESSOR BOWER, in his address to the Section of Botany, placed before his hearers the principles of modern morphology, and discussed the limits of their application. He advocated the establishment of classifications upon purely phylogenetic grounds. The attempt is beset with difficulties of all kinds, but it is the only goal of the taxonomist. Now this transference of our point of view from mere similarity of structure to questions of the origin of each structure brings into still greater relief the ever more complicated problems of homology. When we find that organs, structurally similar, have been independently developed in totally different races, how far can we consider them homologous? Ought we even to call them by the same names? The difficulties are manifest enough in every group of animals and plants; but often they are complicated by an alternation of generations, in which case the use of identical terms for organs that arise at absolutely different stages of life-history is apt to give rise to serious misconception.

"Taking the case of leaves for the purpose of illustration, we may contemplate the following possibilities:—(*a*) A possible origin of two homoplastic series of leaves in the same plant, and the same generation (*Phylloglossum*); (*b*) Two homoplastic series in the same plant, but in different generations (*Lycopodium cernuum*); (*c*) a possible distinct origin of homoplastic leaves in distinct phyla, but in the same generation (sporophyte of ferns, lycopods, equiseta); (*d*) a distinct origin of homoplastic leaves in distinct phyla, and distinct generations (*e.g.* leaves of Bryophyta and of Pteridophyta). Now *Homology* has been used in an extended sense as including many, or even all, of these categories. It seems plain to me that this collective use of the term homology carries no distinct evolutionary idea with it; it indicates little more than a vague similarity; the word will have to be either more strictly defined or dropped. The old categories of parts based upon the place and mode of their origin are apt to be split up if the system be checked by views as

to descent. Comparison, aided by experiment, supersedes all other methods, and the results which follow raise the question of terminology of parts which have arisen by parallel development. In parts which are of secondary importance, such as stipules, pinnae, the indusium, hairs, glands, the inconstancy of their occurrence points to independent origin by parallel development in a high degree; in parts of greater importance, such as leaves, a parallel development may also be recognised, though in a less high degree; in the case of sporangia, their acceptance as a category *sui generis* dispelled the old view of their various origin from vegetative parts; but we must remember that this does not by any means exclude a parallel development also in them, by enlargement and septation from some simpler spore-producing body, though this is not yet a matter of demonstration. There are two extreme courses open to those who wish to convey clearly to others such matters as these: the one is to use a separate term for each category of parts, which can be followed as maintaining its individual or essential identity throughout a recognised line of descent—in fact, to make a polynomic terminology of members run parallel with a polyphyletic development; the other course is to make it clear always in the use of terms applied to parts, that they do not contain any evolutionary meaning, and to use them only in a descriptive sense. Perhaps the former is the ideal method, and it may be a desirable thing, as polyphyletic origins of parts become more established, that the terminology should be brought to reflect at least the more important conclusions arrived at. For the present, the whole matter is still so tentative that it is well to be content with something which falls short of the ideal, and to maintain the usual terms, such as stem, leaf, root, hair, sporangium, &c., as simply descriptive of parts which correspond as regards general features of origin, position, and nature; but with no reference either, on the one hand, to conformity to any ideal plan, or, on the other, to any community by descent—in fact, we shall preserve the original pre-Darwinian sense of these words, which was purely descriptive, and avoid any attempt to read into them any accessory meaning."

Form and Function

"And it was full of bones; . . . and lo, they were very dry."—Ezekiel.

One of the most serious drawbacks to the study of Botany as it is put before us in recent text-books, especially English text-books, is the dry unlifelikeness of the part devoted to Morphology. And especially external morphology, the comparative study of the general form and development of the plant-members, which, completely divorced as it has been from the study of function, reserved for the chapter on physiology, has often degenerated into a succession of

pages filled with theory and bristling with technical terms. Dr Goebel, professor at Munich University, is to be congratulated on the attempt in his recently published *Organographie der Pflanzen* (part 1) (Fischer, Jena) to clothe and breathe the breath of life into those dry bones. Taking his text from Herbert Spencer when insisting on the interdependence of structure and function, and the impossibility of giving any true explanation of natural phenomena without keeping in view this co-operation, he puts before us a system of morphology based on physiology and biology. As in the case of most reformers he sometimes goes too far. Few botanists will follow him, when, for instance, he sets aside entirely homologies of stem and leaf-structures, preferring instead physiological analogies as a basis of terminology.

Notwithstanding such occasional examples of over-zealousness, Prof. Goebel's book is a most useful addition to botanical literature, and should be read and marked by all advanced students and teachers of botany. The subject matter falls under five sections, namely, general segmentation of the plant-body; symmetry relations; difference in formation of organs at different stages of development; young forms, malformations and their significance for organography; and influence of correlation and external stimuli upon form. We are especially glad to note the clear-headed treatment of the section on malformations which have been too much pressed into the service of morphology. Malformations, which by the way cannot be sharply distinguished from variations, follow certain laws, and are either inherited or caused by external factors. The study of monstrosities lends support to Sach's theory of 'Stoff und Form,' which insists that "differences in form of plant-organs are based on differences in material, and that alterations of form are due to alterations in the nutritive processes."

Rind Fungus and Sugar Cane

The Experimental Fields Station at Skerretts School, Antigua, has just issued a report by Messrs Francis Watts and F. R. Shepherd on the results obtained in the experimental cultivation of the Sugar Cane. These are a continuation of those which have been conducted since 1891, and comprise a study of a number of varieties of cane which have been established at the station for six years, an attempt to introduce additional varieties, and a record of results obtained on the applications of various manurial substances to the Bourbon cane in the hope of ascertaining the manurial requirements of the sugar cane under the conditions prevailing at Skerretts.

The chief result seems to be that the Rind fungus (*Trichosphaeria*) is a specific disease, because it cannot be attributed to a deficiency of lime in the soil as some writers have suggested. The disease

often occurs in places where the soil contains carbonates of alkaline earth equal to 40 or 50 per cent of carbonate of lime. As a general rule the authors seem to have found that the addition of Nitrogen either as Sulphate of Ammonia, Nitrate of Soda, or Dried Blood, or of Phosphates (mineral or basic rather than superphosphate) is beneficial, but at present it is very difficult to draw definite conclusions.

Agriculture in the United States

The excellent year-book of the U.S. Department of Agriculture (1897) of which we have recently received a copy shows a useful departure from the usual form. In addition to the miscellaneous papers, eighteen in number, there is a series setting forth the work of the several bureaus and divisions, under the general title "Work of the Department for the Farmer." No better means could be devised of proving to the American people, and peoples generally, the enormous advantages of such a department equipped with the best scientific experts that can be procured, and the great saving to the nation financially. Take for instance the first on the list, the 'Weather Bureau,' the work of which in relation to practical agriculture falls under three heads: (1) The forecast services for predicting storms, cold waves, and frosts. (2) The river and flood service for predicting floods. (3) The climate and crop service for recording and presenting the details of climate and the weekly and monthly conditions of crops. Besides the 150 paid meteorological stations there are no less than 3000 voluntary observers, the majority of whom, under the liberal policy of the Government, have been presented with standard instruments. By a wide distribution of weather forecasts and warnings, together with suggestions for minimising the injury arising from sudden meteorological changes or disturbances, crops, stock or property is saved each year many times exceeding in value the cost of the department.

The department of Botany refers to its work in introducing forage-plants suitable to the various parts of the country, in investigating fungous diseases of plants, in exposing the adulteration of seed, &c. The subject of weeds has been taken up with good results chiefly by preventing their introduction into uninfested parts of the country. Through a large number of correspondents the department is kept informed as to the distribution of the worst weeds, and maps showing at a glance their present range are constructed and kept on file. When information is received that one of these weeds has been found far beyond its known limits the local authorities are advised and the importance of promptly destroying it suggested, together with means by which the destruction can be accomplished. In this way the Russian thistle, which in 1893

damaged the wheat crop of the West to the extent of $3,000,000 to $5,000,000, has been kept out of California. Besides these two there are seventeen other papers by which the different bureaus and divisions equally justify their existence. One of the most useful items of the work of the department is the wide diffusion of its publications. Half a million copies of the present report are distributed, and the total number of publications issued during the past year was 424, aggregating over 6,500,000 copies. Yet the Secretary complains that he cannot nearly meet the growing demand, and asks for an increased appropriation.

Perhaps some day we shall have a Department of Agriculture on similar lines. In the meantime we would advise all who are interested in the application of science in this direction to buy or borrow a copy of the United States Year-book.

Recent Work on the Foraminifera

Since our note appeared in June last a great number of papers have come to hand. Foremost among these is one by R. M. Bagg, on "The Cretaceous Foraminifera of New Jersey" (*Bull. U.S. Geol. Survey*, No. 88). This paper, we believe, may be considered as the first serious paper on the group which has appeared in America, and is, moreover, well illustrated, and well edited. Bagg lists and describes about 110 forms, of which 6 are considered new. The work has been done from a zoological, not palaeontological, standpoint, and deserves warm praise. On the whole, we prefer Chapman's *Vitriwebbina* to the word *Vitrewebbina* used by the author. Chapman's contribution to recent literature deals with a new form from Torres Straits, which he calls *Haddonia*, one of the Lituolidae, related possibly to *Rupertia*. The paper appeared in *Journ. Linn. Soc. Zool.*, vol. xxvi. In the *Journal of the Royal Microscopical Society*, 1898, pp. 258-269, is a paper by that careful writer, F. W. Millett of Marazion, whose work, unfortunately, we so rarely see in print. It is, however, one of a series, which the Microscopical Society may well be congratulated upon having secured. Mr Millett deals with the Foraminifera of the Malay Archipelago, from material from thirty stations collected by Mr A. Durrand. At present only the *Miliolinae* have appeared, but there is promise of a series of especial value, and one which should be doubly welcome to those investigating the structures of Funafuti and Christmas Islands. The region from which this rich material comes was practically untouched by the "Challenger."

Dr Alfredo Silvestri publishes in the *Atti* of the *Accademie di Scienze Acireale*, vol. viii., a "Contribuzione allo studio dei Foraminiferi Adriatici," part 1 of which appeared in 1895. The work is the more valuable as it is the first thoroughly systematic descrip-

tion of the contents of the sands, so laboriously studied by Soldani, and rendered classic by the figures of Plancus, Gualtieri, Breyne and Ginanni. Silvestri also figures a new form of *Peneroplis pertusus* in *Memorie Pont. Accademie Nuovi Lincei*, vol. xiv.; but these one is tempted to regard more as worn specimens than novelties. They are however well figured on two plates, and form a useful contribution to the subject in any case. Carlo Fornasini is still busy, and gives us a beautiful plate of *Uvigerina bononiensis* in *Rivista Italiana di Palaeontologia*, anno iv., one specimen of which shows a double mouth. He also publishes another contribution to the Tertiary Foraminifera of Italy, dealing this time with the Pliocene of San Pietro, in Lama, near Lecco. His third paper, now before us, is "Indice de le Rotaliine fossili d'Italia," published by the Bologna Academy in its Memoires, and which is especially valuable in that Fornasini gives *facsimiles* of d'Orbigny's original drawings of his described species, which have never been published before.

Yet another paper is one by Jan Perner of the Prague Museum who describes and figures in the *Bulletin international, Academie des Sciences de Bohême*, 1898, some very interesting Lituoloids from the Tithonian of Stramberg. Five forms are described, of which three are considered new, and two are insufficiently known to be specifically determined. Mr Charles Schlumberger occupies our attention with two papers, one, in *La Feuille des Jeunes Naturalistes*, on *Involutina conica*, n. sp., an interesting form from the great oolite of Héronvillette near Caen. The specimens were obtained by heating the rock and then plunging it into cold water. His second paper is on a new genus, which he calls *Meandropsina* Mun.-Chalm., though we believe that this is the first appearance of the name in print. The genus resembles *Orbiculina*, is formed of three thicknesses of cells, the centre of which is composed of spiral chambers, starting from an initial sphere, and becoming concentric and circular: this is covered above and below by a layer of vermiform and meandriform chambers. The layer is imperforate, but the last chamber has numerous openings all round the disc. It is of Cretaceous age. Friedrich Dreyer contributes a magnificent monograph on *Peneroplis*, of 119 pages and four double quarto and one single quarto plates. This is a separately published work issued by Engelmann of Leipzig at 12 marks, and the numerous figures show plainly the immense variation among the foraminifera.

The Periodical Cicada

The latest entomological Bulletin (No. 14, n.s.) of the U.S. Department of Agriculture comprises an exhaustive account by Mr C. L. Marlatt of *Cicada septendecim*, the American Periodical Cicad. The bibliography of this famous insect goes back to the year 1633, when

the sudden appearance of a brood was believed to be the cause of a "kinde of pestilent feaver." Records of the insect, during nearly two centuries, over the eastern and central States, have shown that there are two races, a northern with a seventeen-year, and a southern with a thirteen-year life-cycle. A number of broods of each race have been registered, and their distribution being known, the future occurrences of the insect can be accurately forecast for the various districts. Careful observations of the larval and nymph stages have been made, and the changes undergone by the insect during its long underground life have been traced. When development is complete the nymphs of a brood leave the ground almost simultaneously, and an alarming swarm of cicads is the result. The perfect insects live but a few weeks, and are believed to take no food. The female makes cuttings in tree-twigs wherein she deposits her eggs; the newly hatched larvae fall to the ground and burrow immediately. The injury caused by the cicads is almost confined to their egg-laying incisions; though the larvae and nymphs suck sap from the roots of plants, their slow rate of growth and feeding prevents them from doing much damage. The life-cycle of this cicad is longer than that of any known insect, but Mr Marlatt makes the probable suggestion that other larger species of the family might be found to have even longer larval stages, could the course of their generations be accurately followed.

The Larva of Pelophila

In part 2 of the *Transactions of the Entomological Society* for the present year (pp. 133-140), Messrs W. F. Johnson and G. H. Carpenter make a contribution to the neglected subject of the life-history of the Coleoptera, by describing with figures the grub of the ground-beetle *Pelophila borealis*, which they have discovered in Ireland. The larva agrees with those of the beetle's nearest relations —*Nebria* and *Leistus*—in possessing a pair of long, mobile cerci at the hinder end of the abdomen, apparently a primitive character. The head of the *Pelophila* grub, however, is broad and quadrate, and the legs short, contrasting with the rounded head with constricted neck and long legs of *Nebria* and *Leistus*, and in these respects recalling the structure of more generalised carabid larvae.

Flat-Fish of South Africa

Mr J. D. F. Gilchrist, who was recently appointed Marine Biologist to the Government of Cape Colony, is publishing in separate papers descriptions by various specialists of the material which he collects. These are entitled, "Marine Investigations in South Africa. Department of Agriculture, Cape of Good Hope." We have received a copy of a short paper on the Flat Fishes by Mr G. A. Boulenger.

It contains the description of a species new to science, and descriptions of the five other species previously known from the coast of South Africa. The new species is an *Arnoglossus*, and receives the name *A. capensis*. In these descriptions, as in many others published by the systematists of the British Museum of Natural History, generic characters are unnecessarily repeated, and no attempt is made to point out the characters which distinguish the species from its nearest allies. The new *Arnoglossus* is described from a single specimen 16 cm. long. The sex of the specimen is not stated, nor is any mention made of the depth at which it occurred. Considering the interest that has been exerted by the sexual dimorphism of the British *Arnoglossus*, some reference to the subject might have been expected in the definition of a new species of the genus. In the case of the other species, beyond the statement of the specific characters, no details concerning the specimens are given. It is to be hoped that other specialists who undertake the examination of Dr Gilchrist's collections will describe the specimens, and not merely identify them, and will give specific definitions.

A New Ichthyosaurus

A fine skeleton of Ichthyosaurus has recently been uncovered in a quarry in the Lower Lias at Stockton, a village near Rugby. The owner, Mr Lakin, communicated with the authorities of the British Museum (Natural History), to which institution he has presented the specimen, and arrangements were made for carefully extracting it without disturbing the relative position of the bones. The animal lies in a clayey band, which, is unfortunately, unfavourable for the preservation of any traces of the outlines of the soft parts such as occur in the specimens described by Fraas. The only parts of the skeleton wanting are portions of the pelvic and pectoral girdles and some small bones of the paddles. The total length is about 18 ft. The quarry is said to have been visited by thousands of people, most of whom, no doubt, would not have taken the smallest notice of the skeleton had it happened to be in a glass case. The attention of some of these enthusiasts might perhaps be profitably directed to the County Museum where there are many fine fossils.

A New Dinosaur

Another still more important find of reptilian remains has recently been made by Mr A. N. Leeds, whose collections of vertebrate remains from the Oxford clay are so well-known. In this case, a large part of the skeleton of a gigantic Dinosaur has been obtained, including a series of twenty-six caudal vertebrae, sixteen feet long, a hind limb, the femur of which is four feet six inches long, parts of the fore limb and of the pectoral and pelvic girdles. It is

to be hoped that further excavations will lead to the discovery of other bones, and particularly of the skull. The parts at present known indicate the existence of a dinosaurian reptile which seems to be closely related to the American genus *Diplodocus* and to the well-known *Cetiosaurus* and *Ornithopsis* of this country. In fact it is possible that it will be found that all these genera are almost identical.

The Exhibition of Extinct Vertebrates

From the Annual Report of the American Museum for 1897 we learn a good deal as to the methods employed for familiarising the public with the skeletons and external forms of the great extinct vertebrata. In this Report attention is especially called by means of photographs to a skeleton of an Upper Miocene Rhinoceros, in which the perfection of the beautiful methods employed by Mr Adam Hermann are well seen. The entire skeleton is supported by steel rods which pass through the centre of the bones, only the two main supports being visible. This gives a very striking effect, and seems a desirable method, provided the bones themselves are duplicates and not types. We are of the opinion, however, that duplicates and only duplicates should thus be treated, and consider that described or figured specimens, or unique things, should never be sacrificed, for when once a bone is pierced and mounted it is inaccessible to the student, to whom it is of far more importance than the general public, who are quite satisfied with a plaster cast to look at. The plasterotheria of our museums are admirable as teaching objects, but the anatomist wants to handle and examine the real bone, and such should never be maltreated in any way. The mount of *Phenacodus* in the American Museum is an admirable example of what we mean, for there every bone can be removed for purposes of study. Among the plaster reproductions of external forms of animals this Report illustrates *Agathaumus, Hadrosaurus, Naosaurus,* and a highly amusing representation of a combat for the diamond belt between J.L.—we beg pardon—*Megalosaurus* (*Laelaps, Dryptosaurus*) *aquilunguis* and another of its kind, which is very real, and would, could it possibly be seen at the Aquarium, draw immense houses. The attitude of defence of the recumbent dinosaur is very striking, and the terrific lunge possible of the hind legs might fairly convert the attacking creature into a constellation.

Cretaceous Rocks in West Greenland

David White and Charles Schuchert accompanied the Peary Arctic Expedition of 1897 to the Nugsuak peninsula, West Greenland. They have now published a paper describing the Cretaceous series of that locality (*Bull. Geol. Soc. Amer.*, ix., pp. 343-368, pls. xxiv.-

xxvi., June 21, 1898), and the following are their chief conclusions. The Cretaceous and Tertiary rocks lie unconformably on a hilly basement of old crystalline rocks and early Cretaceous basalts, and reach at Atanikerdluk 3040 feet above the sea. Along the north side of the peninsula the Lower Cretaceous beds have an easterly dip, although the higher beds appear towards the west, probably in consequence of faults. The sediments appear to have been derived from the east, in which direction are few marine but some freshwater fossils. The deposition of sediment seems to have been continuous in some portion of this region throughout Cretaceous and early Tertiary times, although minor movements and erosion may have affected the beds before they were covered by the Tertiary basalt cap. The entire thickness of the sedimentary rocks is over 3500 feet.

These beds were divided by Heer into four series, on the basis of their vegetable contents. Of the lowest of these, the Kome series, developed on the north coast of the peninsula, a thickness of probably not over 700 feet is exposed above tide. The discovery of additional dicotyledons in the Kome series, from which hitherto only *Populus primaeva* was known, and which was regarded as Urgonian in age by Heer, casts serious doubt on the reference of those beds to so low a stage in the Lower Cretaceous. The flora as a whole is, however, to be compared with that of the Virginian Potomac formation, with some, perhaps the upper, portion of which the Kome series is probably synchronous.

The Atane series, hitherto not positively known on the north shore of Nugsuak peninsula, is clearly present at Ujarartorsuak with characteristic Atane plants. Farther west, at Kook Angnertunek and Niakornat, the dark homogeneous shale series probably represents both Atane and Patoot members of the Upper Cretaceous, since of the marine organisms found here some are identical with those occurring at Ata and Patoot, the typical localities for the two divisions of the Upper Cretaceous. The marine invertebrates from the Atane series, which Heer correlated by means of fossil plants with the Cenomanian of Europe, strongly indicate that the series is to be correlated with Fort Pierre and Fox Hills or Montana formation of the western United States. By means of its fossil plants the Atane series is so closely related to the Vineyard series of Martha's Vineyard, the Amboy clays of the Raritan region of New Jersey, or the uppermost Potomac of Alabama, as to furnish strong reason for the belief that the middle one of Heer's groups is the Greenland contemporary of the Amboy clays. The Patoot series, which appears lithologically and stratigraphically to be inseparable from the Atane series, contains at the same time many plants common in the upper part of the Amboy clays, with others allied

more closely to the higher Cretaceous floras, such as that of the Laramie. The Patoot series may perhaps be safely interpreted as constituting a palaeontological as well as sedimentary transition from the Atane series to the Tertiary. The thickness of the Atane and Patoot series (Senonian) is not less than 1300 feet and may considerably exceed this.

Tertiary and later Geology of W. Greenland

Many plants from Atanikerdluk were referred by Heer to the Miocene, but are now generally admitted to be Oligocene, and Messrs White and Schuchert suggest that they may even be of Eocene age. They further remark that the distinction between the floras of Heer's three Cretaceous series rests largely on minutiae of systematic description that cannot be upheld, a view that will doubtless be shared by most palaeobotanists.

There is some evidence for Tertiary erosion west of Niakornat. After this the entire region was covered by a great number of superimposed, approximately horizontal, non-columnar basalt beds of varying thickness and of great extent. Frequently 3000 feet of this basalt cap remains, while at Kilertinguak (6250 feet above tide) over 4000 feet is preserved. In certain regions numerous dikes intersect at varying angles the Cretaceous, Tertiary, and even the lower portion of the basalt cap, and are frequently found both forking and intersecting. Intruded basalts are not rare, especially in the Tertiary. The peridotite intrusive beds, about 350 feet thick, behind Kaersut, are probably of Tertiary age, as are also the other high intercalated basalts. At the time of the great elevation of the region, probably in the late Tertiary, the basalt cap, which, judged by the development on Unbekanntes Island, may have exceeded 7000 feet in thickness, most probably extended northwards in an unbroken sheet from the south of Disko Island to beyond the Svartenhuk peninsula, a distance of 250 miles.

The dissection of this great basalt sheet, the development of the Vaigat, the Umanak fiordal system, the isolation of Disko—in fact, approximately the present land topography of this coast—were accomplished at a much greater elevation during Pleistocene time. Evidence of post-Pleistocene subsidence, with Arctic climatic conditions, is found in the presence of recent Arctic marine shells occurring in terraces at an elevation of from 100 to 150 feet above tide. In the old crystalline region, much farther south, the terracing is said to extend to 300 feet above tide. The extent of the more recent uplift is not known, since the retreat of glaciers, the inundation of ancient dwelling-sites, and the records of tide-gauges point to present downward movement observable within historical time.

I

The Species, the Sex, and the Individual

Part II.

I HAVE been discussing characters that are related to sexual courtship, but among characters confined to one sex there are others which are connected with other actions. For instance, there are structural peculiarities which are only employed in combat. Among the most highly developed of these are the antlers of stags. It cannot be disputed that these are the special, apparently the only weapons of the stag, and that there is no stag in a state of nature which does not regularly follow the practice of duelling without any variation in the arms employed. But those who consider that the evolution of the antlers is sufficiently explained by the constant victory and survival of the stags which have them most developed, leave out of view important problems:—Firstly, why do the antlers only begin to develop when the stag becomes mature; Secondly, why are they renewed every summer, and drop off again in spring? In relation to these problems it is at least significant that the males only fight when they begin to breed, and, when mature, only in the breeding season, which is limited to the autumnal months. As the fighting of stags is fierce and frequent, it is quite possible that the irritation due to butting with the forehead was the exciting cause in the beginning, and has been ever since, of the remarkable outgrowth of bony tissue which forms the antlers. If this were so, it would be physiologically intelligible that when the stimulation ceases at the end of the butting season and the circulation becomes less active, the bone should cease to grow, should become dry and brittle, and then the antlers should either drop off of their own accord, or be intentionally broken off by the stag. Next season a renewal of the fighting would cause a renewal of the growth. My theory is, that stimulations periodically repeated, physiologically cause periodical phenomena of growth, and that these rhythmical processes of growth repeated in successive generations ultimately become hereditary. It is, or has been, a current belief that effects so caused are not inherited; but such inheritance has not yet been proved to be impossible. I am only attempting to show that the facts seem to lead inductively to the conclusion that structural evolution has

been largely determined by the direct influence of stimulation on growth.

In the reindeer, and in the bovine animals, the horns are developed in both sexes, and appear at an earlier age; those in the reindeer, however, are also periodically shed, and consist of bare bone, while those of the Bovidae are permanent and are encased in cornified skin. I do not know whether the young males and the females of the reindeer fight, or whether there is any other special habit in them to explain the development of their horns, but in the bovine animals it might be suggested that the same stimulus of butting is applied less violently and not with the same regular periodicity, and therefore has led to more permanent growth.

A brief consideration of the third kind of structural differences is now to be undertaken, namely, differences in the structure of the same individual at different periods of life. This is, in some respects, the most important of the three kinds I have defined, for it is inseparably connected with the other two. We cannot investigate the origin and cause of the differences between kinships, or between members of the same species, without studying the transformations of the individual, for these differences arise as alterations in the development of the individual.

Now the embryos of the higher vertebrates all exhibit certain characters in common, in the presence of gill-arches and gill-slits, and in the origin of the limbs as bud-like outgrowths. The great embryologist of the beginning of this century, Von Baer, whose studies were directed principally to the higher vertebrates, formulated the generalisation that animals of different classes resembled each other closely in the earlier stages of their development, and diverged more and more as they progressed toward their final form. This remains true of the higher classes of vertebrates,—reptiles, birds, and mammals. When the doctrine of evolution became paramount, and it was seen that the comparative anatomy of the higher vertebrates obviously pointed to their common derivation from ancestors which were essentially fishes, the resemblance and the structure of the embryos were attributed to the retention in these embryos of the essential characters of the fish. The generalisation of Von Baer was therefore changed into another, to wit, that in development the individual passed successively through the stages of its ancestors to arrive at its present final condition. Haeckel gave great publicity to this doctrine, calling it the biogenetic law, and formulating it in the terms that ontogeny, or the development of the individual, is a repetition of phylogeny, or the evolution of the race. The late Professor Milnes Marshall still further popularised and established the principle by embodying it in another phrase, namely, that the

individual in its development is compelled to climb its own genealogical tree.

A more comprehensive and more accurate study of the facts of development throughout the animal kingdom has shown that this law is by no means universal. It is true in regard to a certain number of facts in the development of the higher vertebrates, but it is not the whole truth about them, and it is contradicted by a great many other facts even in the development of reptiles, birds, and mammals. For example, snakes are characterised by the absence of limbs. In some snakes rudiments of the hind limbs are present in the adult condition, but in no snake has any trace of the fore limbs been discovered in any embryonic stage. Yet we cannot doubt that the ancestors of snakes possessed two pairs of limbs. Again, there can be no doubt that the wing of the bird has been evolved from a limb with five digits like that of many reptiles, but the wing contains only three digits, and the most complete embryological investigation has only succeeded in discovering small and very doubtful rudiments of the lost two. The ancestors of birds had teeth, but no trace of teeth has been found in the embryo. In the horse again there are traces of the second and fourth metacarpals and metatarsals in the limbs in the adult, but examination of the development has shown that only the merest vestiges of the second and fourth digits are ever formed, and of the first and fifth none at all.

Balfour has expressed the general result of observation in the statement that ancestral stages are liable to be omitted from embryonic development by abbreviation, and to be obscured or replaced by new characters in free larval development. He also suggested that the retention of the branchial arches and clefts in the embryo of higher vertebrates was due to the fact that these structures were functional in the larval stage of the amphibian ancestors of these vertebrates after they had become rudimentary in the adults, and that the limbs in snakes had completely disappeared because there was no such advantage in their retention at a particular stage.

Mr Sedgwick has lately elaborated this last suggestion into the general theory that ancestral characters are only retained in the embryo when the ancestral condition was once a larval condition which has more recently become embryonic, in consequence of the retention of the larva in the egg or within the body of the mother until after its metamorphosis.

These are numerous instances of the truth of Mr Sedgwick's theory, but it is not a general theory of individual development. The general theory will be found to correspond to that which I have indicated in the case of the structural differences between groups of species and between individual types in the same species.

The general truth is that when the habits and conditions are different at different periods of the individual life, then, and to a proportionate degree, will the structure of the individual be different during those periods. It may be said that this is merely another way of stating the principle of adaptation as the result of natural selection, the most advantageous variations being selected at each stage of life separately. But my contention is that, if there is not sufficient evidence of the occurrence, apart from the influence of habits and conditions, of the variations necessary to explain the other two kinds of difference, still less have we proof that the changes in the structure of the individual at different periods of life have been independent of the direct influence of the changed conditions. To my mind the phenomena of metamorphosis can only be explained on the principle that the different conditions acting on the individual at different periods of its life give rise to and determine the direction of the modifications which characterise the successive stages of the individual structure.

This matter again can only be studied in actual instances. The most familiar case is that of the frog and other Amphibia. We can have no doubt that the air-breathing Amphibia were evolved from fishes, though we may not be able to say exactly what kind of fishes. We have, however, various transitional or intermediate forms in the lower Amphibia and in the Dipnoi or lung-fishes, which breathe air to some extent. Now, how can we conceive the conversion of a single individual fish into an air-breathing creature, apart from the change of conditions, the breathing of air? It is true that the blood can and does secrete gases, oxygen and other, into a closed air-bladder, but the structural arrangements connected with the action of lungs, cannot be conceived apart from the respiration of atmospheric air. We know of plenty of cases in which, the water being scarce or foul, fish have become capable of breathing air, in one way or another, but we have no evidence of the occurrence of variations in adult life tending towards air-breathing structures in fishes which are never exposed to the air. We do not find them, for instance, in fishes that live on the sea-bottom or in the ocean abysses. When the fish is exposed to the air at a late stage of its life, then its structure undergoes modification, first into a lung-fish breathing both air and water, or into an amphibian that retains its gills throughout life. Afterwards such a form spreads into places where water is still scarcer, and it becomes still more modified, so as to breathe air altogether, and to crawl about on land.

But, at the same time, the young aquatic stage or larva is being modified. If we suppose that the tadpole resembles the ancestor of the frog, it follows that that ancestor was destitute of paired limbs

and of fin-rays, and that the terrestrial form of limb, transversely jointed into three segments and divided at the extremity into five digits, was not evolved from the fin of a fish, but was a new organ. Such a view is very improbable and by no means inevitable. It is much more reasonable to suppose that the terrestrial limb was evolved by the modification of the fin of a fish. The tadpole has lost its limbs, because in its short life their use has become diminished. It does not sustain itself in the water, but fixes itself to plants by means of its suckers, and moves from one place to another by violent strokes of its tail. Its habits have been almost as much altered as those of the frog, and its structure has been determined by its habits. Thus from an ancestral fish has been evolved a creature passing by a well marked metamorphosis from a larval aquatic stage to an adult terrestrial stage, and in each of these stages it has become very different from its ancestors.

The original reptiles were derived from the Amphibia by a change in the character of their eggs, which acquired large yolks and were enclosed in tough shells. Within the shell the larva was retained, never being set free in the water, and thus for the first time terrestrial vertebrates became entirely independent of a liquid medium. The embryo in the latter evolution of terrestrial vertebrates has undergone various modifications, but the condition in which we find it at the present day is the original larval condition of the amphibian ancestor, except so far as it has been modified by the conditions of development within the egg-shell or the uterus. Thus the course of development in the higher vertebrate is not to be explained by the law of recapitulation, according to which transient embryonic stages represent ancestral structures, but by the fact that the embryonic stage is a larval stage which passes through its metamorphosis before hatching or birth. The larval stage and the metamorphosis were originally determined by the temporary conditions of life in the individual, and the persistence of larval characters in the embryo is due to the fact that there has been nothing in the conditions of embryonic life to change them.

Let us turn now to another instance, namely, the transformation of the flat-fishes. Perhaps it will be thought that there can be no excuse for throwing doubt upon the accepted doctrine that the larva of these fish swimming upright in the water with an eye on each side of its head, repeats in individual development the conditions of the ancestor. But a more careful study of the facts shows that this doctrine is erroneous, or at least only a partial truth, and it must be modified to agree with the state of knowledge at the present time. A brief summary of the facts will be sufficient to prove this.

The flounder when first hatched is a minute larva not quite $\frac{1}{8}$th in. in length. The right and left sides are perfectly similar to

one another, and it swims vertically in the water. But it has no fin-rays and no bones, a continuous fin-membrane passes along the edge of the back round the end of the tail. The conversion of this larval form into the fully developed flounder takes place when it is from two to three months old, and about half an inch long. When the bones and fin-rays begin to develop, the left eye rises first to the edge of the head, and then passes completely over to the right side. At the same time the little fish begins to lie on its side on the ground, and loses the power of sustaining itself in the water. With slight differences in details, the development and metamorphosis of other species of flat-fish are similar. The early condition of the flat-fish therefore is not that of any fully developed fish at all, but of a fish larva without bones or fin-rays. It is in all respects similar to the larvae of other marine fishes, for instance, to that of the mackerel or that of the cod. When the bones begin to develop the eye begins to become asymmetrical, and we have not the ancestor but the flat-fish. We do not know at present whether the elongated fins along the dorsal and ventral edges had the same form in the ancestor, and we have reason to believe they had not so great an extent, yet they are developed directly, not by gradual increase. The true reading of the matter therefore is, not that the ancestral condition is repeated, but that the larval condition of the ancestor is retained, because the larva is still hatched and still lives in the same way; but the structure after metamorphosis is different because the adult fish has acquired different habits. On the theory of natural selection we must suppose that those individuals have been selected whose eyes were most symmetrical in the larval stage, and most asymmetrical in the adult condition. But we have no evidence that among symmetrical fishes individuals occasionally occur in which one eye moves up towards the edge of the head during growth. Even if slight variations of such a character were proved to exist, it would be difficult to believe that they would be great enough to make any difference to the fate of the individuals possessing them when the fish took to lying on the ground. The theory of independent variation and selection as applied to flat-fishes is unsupported by evidence, while the conclusion that the metamorphosis of these fishes is the direct result of the change of conditions is in harmony with all that we know of the effect of physical conditions on individual organisms.

In these two cases, that of the frog and that of the flat-fish, the larval condition is either unmodified or less modified from the ancestral condition than the adult. But in numerous other cases the larva has been modified in adaptation to new conditions while the adult has remained nearly the same. This is particularly conspicuous in many insects. I will not discuss at length the question of

the tracheal gills of aquatic larvae, for, although they are probably secondary adaptations in the larva, there are some who regard them as representing an ancestral series of organs from some of which the wings were derived. But if this be the case, the entire absence of wings and tracheal gills in the terrestrial larvae shows that the latter by no means recapitulate the ancestral history. Again, if the legs on the abdomen of the caterpillar behind the three pairs of thoracic legs are in any way related to the abdominal appendages of the ancestor, it is all the more certain that the maggots of the flies or of the ants, bees, and wasps, having no legs at all, cannot resemble the ancestor. In such cases the structure of the larva corresponds to its mode of life, and is much more different from any possible ancestor than is the adult. The individual does not here climb its own genealogical tree, unless it may be said to begin at the top and climb downwards. As for the origin of the modifications in the young stages, we have no evidence that their appearance was independent of the conditions; the fact that the special structure only lasts as long as the special larval habits last, suggests strongly enough the direct dependence of the modifications on the conditions of life.

To sum up the argument of which I have attempted to give an outline, its main points are these. Selection assumes the occurrence of variations: the variations must either be similarly indefinite and promiscuous in all cases, or they must be different in different cases —that is, in different species, different sexes, different stages of life. If they are different in different cases, then selection is a very unimportant matter, for the chief questions are evidently what are the differences and what made them different. To deny that the variations have always been different in different cases is to deny the most evident facts: such denial might be possible when we consider only the difference between species, but is impossible when we study the differences between the sexes in the same species and between different stages in the same individual. In all cases the variations correspond to differences in habits and mode of life, and in many cases are of the same kind as the changes known to be produced in the individual by special stimulation or special activity of organs: this is true of many and probably of all cases of adaptation. The general conclusion is that adaptation is not produced indirectly by the selection from indefinite variations, but directly by the influence of stimulation in modifying the growth of the parts or organs of the body.

J. T. Cunningham.

1 Morrab Terrace,
Penzance.

II

Bees and the Development of Flowers

DARWIN and Dr Russel Wallace maintain that the bright colours of flowers are due to insects, and this view has, till recently, been accepted by most biologists. But difficulties become apparent as soon as the methods of insect workers are closely investigated. The result of such investigations has been that some naturalists (among them Mr G. W. Bulman, writing in *Natural Science*, Aug. 1897) have come to the conclusion that the colours of flowers have arisen quite independently of insects and that they have yet to be accounted for on Darwinian principles.

A priori, if it be granted that the Darwinian hypothesis affords a satisfactory explanation of other phenomena of the animal and vegetable worlds, it seems unlikely that it should leave the colours of flowers unexplained. Moreover, flowers that are invariably fertilised without insect aid are almost all of them dull and inconspicuous. The young cones of the larch are an exception. In these the colour may, possibly, be looked upon as a by-product of the physiological activity of the plant. The more striking blossoms, elaborate in form and coloration, cannot possibly be mere by-products.

The difficulty of explaining the colours of flowers, though by no means insuperable, is very real. Insects can never produce a new species unless during each journey from and back to the hive they keep to the same sort of flower. That they show a remarkable constancy is undeniable. When at work upon dandelions they will not wander to a neighbouring narcissus. In thus keeping to flowers of the same make, they are consulting their own interest: they can extract the honey with greater speed than if they wandered to flowers of a different build. Frequent practice at the same exercise produces great dexterity of limb and proboscis, and the work goes merrily on. There is but little of the tentative buzzing and reconnoitring that is unavoidable when a bee is investigating an unfamiliar flower. And thus it is to a bee's own interest not to transfer pollen to a flower of a different genus or of different family. But she is often tempted to go from one variety to another, or to a closely allied species, and she does so without scruple. Thus it is just where her constancy might seem most needed that it breaks down. When new varieties are arising, the operation of bees comes

in, to swamp them, if possible, by inter-crossing, and so prevent them from developing into species.

How bees fail as species-makers, may be seen from the following examples. In a field of buttercups there are often two species in blossom side by side, *Ranunculus bulbosus* and *R. acris.* The former begins to blossom a good deal earlier than the latter, but the flowering times of the two overlap. If you watch a bee among these, she will often for a time keep to one species. *R. acris* stands a good deal higher and, owing to this, she will for a while perhaps pass over *R. bulbosus.* But before long she will often change her level and busy herself with the lower-growing species. In a bed of mixed polyanthus flowers she may often be seen going from one colour to another, heedless of the claims of polytypic evolution. The same thing takes place when she is busy upon rhododendrons and columbines of slightly different or even widely different tints. These instances of infidelity to colour or species I select because I have recently observed them. There is no doubt that they overthrow the theory that insects by their constancy have been makers of new species. It must be owned that bees, in spite of their great reputation, dating from the days of Aristotle, are great blunderers. Still I cannot but believe that to bees and other insects are due all the brilliant colours of wild flowers. The transference of pollen from variety to variety is an undoubted fact. But what if it produces no effect? A number of French botanists, wishing to prove that evolution was a myth, have made experiments,[1] during a number of years, showing that even varieties distinguished by what appear the most trifling differences, are inter-sterile. This discovery is in reality no blow to evolution, but by the irony of fate it comes in very opportunely to help the Darwinian theory. The inconstancy or defective constancy of bees is of no consequence, since most varieties and, possibly, all species of wild plants are inter-sterile or, if they are not absolutely inter-sterile, their own pollen is prepotent, so that when two pollens are put on one flower, that which represents its own variety or species alone takes effect. In addition to all experiments made by botanists, we have those made by the bees themselves: they are constantly doing their best to inter-cross species and varieties, and we know that the species and varieties remain distinct.

To account, then, for the colours of flowers we have the proved colour-sense of bees—Sir John Lubbock has tested it by experiment: they are attracted by brilliant blossoms and, therefore, it has been to the interest of plants, in order to obtain cross-fertilisation, to produce conspicuous flowers. Every variation in the direction of

[1] An account of these experiments may be found in Romanes' *Darwin and after Darwin*, vol. iii.

increased brilliancy has been of advantage. But the bees for whose favours the plants were bidding would have ruined all, had not the plants on their side had a very marked characteristic: infertility with varieties of the same species, or with other species of the same genus. The bees have constancy enough to ensure frequent cross-fertilisation between plant and plant within one variety or species: but, intent upon their own business, they take no trouble to put barriers between new varieties. This work is done by the plants themselves. The varieties, at any rate all the large number that have survived, have kept themselves apart. Through this inter-sterility the bees have been gardeners who had each variety and each species in an isolated garden, and for whom each new variety as it arose proceeded to isolate itself without any trouble on their part. They with their colour-sense and the plants with their preference for pollen from one of their own species, or even for pollen from one of their own variety have, working together, given us all the colours, shapes, and scents of flowers.

F. W. Headley.

Haileybury College.

III

The Eskers of Ireland

Part II.

THE various theories which have been advanced to account for eskers may, I think, be reduced to two classes—

The first class will contain those held by geologists who, while differing in matters of detail, agree that the eskers result from marine agency—tides and currents.

The second class will contain those propounded by writers who see their way to dispense with marine action, and who find in the agency of glacier-ice the origin of all manner of drift deposits.

The Neptunists—a term which will serve to indicate the upholders of the marine agency theory—are well represented by Jukes and Kinahan, who have the advantage, as regards the eskers, of minute practical acquaintance with the geology of Ireland.

The Glacialists are represented by Hummel and Geikie—the former belonging to the Geological Survey of Sweden, and the latter to the Geological Survey of Scotland.

I begin with J. Beete Jukes, who was for many years, prior to his death in 1869, connected with the Geological Survey of Ireland. In his opinion the 'ridges' "seem to have been formed by the piling action of two opposing currents, or to have been heaped up in the eddy at the margin of the currents running in different directions."

But it is in this 'piling-up action' that the difficulty lies. Irregular heaps or mounds of sand, gravel, or shingle may have been got together in this way, although it would not be so easy to account for the stratification observable in the true eskers. But when we come to consider the long narrow ridges of the railway embankment type the difficulty becomes really formidable. I have no doubt that Jukes is correct in saying that "many of the eskers were perhaps similar to harbour-bars in their mode of formation, and may be directly related in this way to the valleys running into the neighbouring hills, which must, of course, have formed bays or harbours during some part of the last slow rising of the land above the sea."

In Geikie's edition of "Jukes' Manual" of Geology, there is a note on p. 712, citing, as an excellent example of an old 'harbour-bar,'

the case of the Seven Churches, Co. Wicklow: "All the ruins are on a bank of drift, stretching across the main valley, and formed partly of the detritus from that valley, but chiefly perhaps from the other steeper and narrower valley, which must at one time have emptied its drainage into the old harbour just above this point, and brought down the detritus, of which the tidal currents formed the bar."

There are, I have no doubt, drift formations in other places, which may have originated after the manner of the 'harbour-bar.' But we must bear in mind the well-marked difference in form of these accumulations and the esker proper.

"Others, however," continues Jukes, "especially those numerous ones which run in various directions all over the great central plain of Ireland, can only have been formed in the open sea by the action of different currents, as that sea became shallow in consequence of the elevation of its bed." Now, as it appears to me, this statement would go further if we could suppose the bottom of this shallow sea already thickly strewn with the prepared materials—the sand, gravel, clay, and rock fragments. As the waters would become shallow, these deposits would more and more come within reach of the surface currents, and, doubtless, would in many places become heaped up in mounds or ridges of some kind. But I am not sure that this is exactly what Jukes intends to lay down.

The 'counter-current' theory has been worked out with boldness and clearness by Mr G. H. Kinahan, in his interesting "Manual of the Geology of Ireland," chapter xiv. In this work there is no uncertainty like that just mentioned. The author includes the eskers among "the moraine drifts that are undoubtedly post-glacial." These drifts he divides into periods corresponding with alleged pauses in sea depression "at or about the 300 feet contour line," "at or about the 100 feet contour line," etc.

The margin of the esker sea, he explains, "will be found in places on the hills sometimes as a shelf cut in the sides, at other times as a beach accumulation." But, as Geikie has shown in the case of the Parallel Roads of Glenroy, near Fort William, in Scotland, the notches and old beach lines may have resulted from fresh water dammed up in the mountain valleys by the obstruction of great glaciers in the plains adjoining. At one time Geikie held the same opinion as Kinahan regarding the marine origin of the eskers. "But," he writes, "having since seen reason for believing that the sea has had no share in the formation of the Scotch gravel ridges, which are in the same category as the Irish eskers, I now look upon the latter as having been heaped up principally by the action of sub-glacial waters during the final melting of the confluent glaciers." This goes a long way towards reducing the esker sea to a myth.

In some places on the low ground the esker sea drifts (as Kinahan terms them) are spread out in undulating sheets, as in the County of Kildare. The famous Curragh may be cited as a tabular deposit of such materials. Now all these deposits point in the clearest manner to 'the rushing of great waters'; and if we only consider the thaw of such an ice-sheet, as all admit to have existed, we can have perhaps floods and rushing waters enough for any purpose, without calling in the aid of subsidence beneath present sea-level.

Mr Kinahan does not, however, forget that the formation of the eskers is a disputed point; but he thinks it probable that they are modifications of the banks and shoals which accumulate at (1) the colliding, and (2) the dividing of the flow-tide currents of the esker sea, similar to those that are found in the seas around Great Britain and Ireland at the present day. He examines three contemporaneous instances of the 'colliding' of currents giving origin to bank formations:

(*a*) In the Irish Sea, in the vicinity of the Isle of Man, there is a meeting of the north and south flow-tide waves, or a 'Head of tide.' Where the tidal currents meet they neutralise, forming a mass of currentless water that simply rises and falls, depositing silt and other materials.

(*b*) There is a 'Head of tide' in the Straits of Dover; and

(*c*) In the German Ocean, between Norfolk and Holland.

But in the Straits of Dover and in the German Ocean the meeting is not precisely a case of 'colliding,' as the currents pass for some distance; and at their edges, or their junctions, long banks of gravel and shingle accumulate.

It is also found, says Kinahan, that long banks of gravel and shingle may form at the dividing and splitting up of the flow-tide currents. We have here some resemblance between currents at sea and rivers on land. Just as the river-flow, dividing at the outlets, makes a deposit which may eventually become a delta, so does the ocean-current where it parts produce an accumulation varying in its constitution, extent and form with the nature and supply of materials affected by the moving mass of waters.

As an illustration Kinahan cites the following: From Greenore Point (Co. Wexford) a main current runs northward up the Irish Sea, while secondary currents break off into Wexford Bay; and at the junction of these currents with the main one there are long banks between Greenore Head and Wicklow Head. Similar results, he adds, must have occurred in the esker sea. He applies his theory in this way:

The flow-tide wave entering at Galway must have sent a main current eastward to the coast between Drogheda and Dublin.

Southward of this current there would be a bay, somewhat similarly circumstanced to Wexford Bay, off which banks would form between Galway Bay and Dublin, that is in the line of country occupied by the principal eskers.

That the author was himself sensible of the difficulty already alluded to, is clear from the words above quoted: "The typical eskers are very unlike shoals." By the conflict, or the separation, of flow-tide currents you can have shoals and flats and mud-banks, but can you have an esker like one of those already described? Let anyone put the question to himself as he stands beside one of these ridges, or walks its road-like top, for only in such circumstances can the difficulty be fully realized.

Kinahan, however, goes further, and explains how subsequent denudation may have played a part in the modelling process.

Although it cannot be affirmed, yet it appears possible (he says) that as the sea shallowed, and the shoals and banks became 'awash,' the current should have the power of changing the massive banks into narrow ridges, for at the half-tide or 'awash,' portions of banks, or in the shallow places where two currents collide, there are esker-like ridges as St Patrick's Bridge between Kilmore and the Saltees Co. Wexford.

But with all this before me I am unable to account to my own satisfaction for the form of the esker proper. It is not easy to conceive how by any process of marine or aërial denudation great massive banks could be attenuated to the slender figure of an esker running for miles like an artificial earthwork. One may ask how the denuding forces could waste all but the back-bone, and yet spare the latter which was, after all, no more likely to resist erosion than the rest of the drift matter.

In the first edition of his Manual, Mr Kinahan attached particular importance to the 'Head of tide' origin of banks, etc. But at a later period he abandoned this in favour of what I have sketched as the 'Cross-current theory.' He believes that, allowing the marine origin of eskers, the various details and complications of the drift formations of the central plain of Ireland could be explained by the 'colliding' or meeting of the flow-tide currents branching from the main, with these coming through the straits—now valleys—in the surrounding hills, as also the different eskers to the north of the main current.

It is indeed remarkable that the great eskers of the central valley are opposite some great gap or valley which, on the hypothesis of an esker sea, was at one time a strait or channel. For example, the Parsonstown esker is opposite the great gap of Roscrea between the Slieve Bloom mountains and the mountains of North Tipperary. The East Galway groups are in relation with the open-

ing between the Silvermines and the Slieve Boughta mountains behind Portumna. Last, not least, the great group of drift ridges and hills at Esker College, Athenry, is situated near the mouth of the great strait which occupied the valley of Gort and Lough Cooter between the mountains of South Galway and those of North Clare.

Dr James Geikie remarks that Mr Kinahan was the first to point out the relation between these groups of post-glacial mounds and ridges with the openings of valleys branching off from the great central plain; and the circumstance is further noteworthy as forming (accidentally, I think) a connecting link between the two sets of theories represented by Kinahan and Geikie respectively. While Kinahan looks to the 'colliding' or dividing of flow-tide currents, Geikie finds all that is necessary in the melting away of 'confluent glaciers.' The latter regards the explanation which Mr David Hummel gives of the åsar of Sweden as applicable to the drift formations of Scotland and (as he believes) to the analogous formations of Ireland, including, of course, the eskers.

From the observations made by him on the åsar and other drifts of Sweden, Hummel concludes that the facts indicate the agency of running water, and the direct action of glacier-ice; and he comes to the conclusion that the åsar have been formed in tunnels underneath the dissolving ice by running water introduced through crevasses, etc., acting on the ground moraines of the great confluent glaciers which covered Sweden during the glacial period. Geikie adopts a similar explanation of the kames of Scotland, and remarks that the theory to some extent resembles Mr Goodchild's view of the origin of drift deposits in general.

Dr N. O. Holst has proposed another explanation of åsar, which he supposes to have been deposited in superficial channels licked out of the ice-sheet by the water derived from the melting of the inland ice. The materials were, as he believes, derived from the melting ice in which they had lain embedded. Geikie rejects the explanation for these reasons:—

(1) Because åsar, eskers, and ridgy kames are not so continuous as they must have been had they been formed in superficial river channels; and

(2) Because we have no reason to believe that the ice of the old extinct glaciers was more thickly charged with debris than the present ice-sheets of arctic and antarctic regions.

Moreover, it occurs to me that, while it is by no means impossible that morainic matter may have collected in such grooves, it is very unlikely that it would come to *terra firma* without disturbance of stratification. The groove would be narrower at bottom than at top, the reverse of what we find in the esker.

Hummel's theory, as adapted by Geikie, while open to a share

of the objections, will perhaps be found to apply to Ireland as to Scotland and Sweden; it will, at all events, be found to explain some phenomena not easily reconcilable with other theories. Take the case of the large erratics frequently perched on the top or sides of eskers and similar deposits. When I have read that such blocks had been borne by ice-rafts which, stranding on the eminences, there melted, and dropped their burdens, I have asked without being able to find an answer satisfactory to myself: How could so slender and fragile a structure survive the rude impact of an iceberg? On Hummel's hypothesis I can satisfy my own mind at least. I can understand how crevasses would wear and widen into tunnels,—how the water flow derived from the melting ice would sweep the morainic matter into these tunnels,—how the melting would vary with the season,—how the solid matter would vary from time to time with the force of the water,—how there would be strata dipping towards the sides which would wear away unequally at different sections,—how even on the same line of crevass we may find a well-formed ridge, a mound, or a heap of morainic matter,—and how the greater blocks would remain on the ice-surface, till let down on the surface or side of the ridge. I am not sure that we can so easily account for the roundness and smoothness of most of the pebbles as we could on the hypothesis of marine action. And I am still at a loss to know why the true esker is confined to the comparatively narrow midland zone.[1] Surely there were crevasses and morainic matter elsewhere, and the great thaw would be general and pretty nearly equal all over the area of Ireland. It is easy enough to conceive that the greater part of the products of glaciation would finally become scattered about, levelled, or, in some places furrowed, by the floods which would cover all the low-lying parts of the country with deposits of sand, gravel, and shingle. Perhaps it may yet be shown that eskers were formed in other parts of the country, but were destroyed by local glaciers of later date, or by other causes. In parts of Ulster, Co. Monaghan for instance, there is abundance of limestone gravel in hillocks and mounds. That there are no long narrow ridges at present does not, perhaps, justify the assumption that there never have been any there. The preservation of an esker is, or ought to be, not less a subject for wonder than its original formation.

I have already partly described the great drift formations at the College near Athenry, and I give some further details, which may afford additional illustration of Hummel's theory. This group consists of two great parallel ridges running westward from the

[1] Some well-formed eskers may be seen adjoining the railway between Tuam and Claremorris. The locality may, however, be regarded as a portion of the great midland plain.

college (which enclose on two sides the lawn or playground), and a series of immense mounds or hills to the south-east, east, and north-east. These sand-hills form a miniature mountain system with valleys, and some curious bowl-shaped depressions. Seen at a little distance, in the twilight they may easily be mistaken for a veritable mountain-chain with sierra-like crest. But there is no appearance of rock, the whole consisting of drift deposits, mainly limestone gravel and sands, with a surface which in a 'dropping' (*i.e.* rainy) season supports a fairly good sheep pasture, and forms burrowing ground for myriads of rabbits. Apart from these ridges and sand-hills the country around is flat, and less than a century ago must have been almost entirely covered with deep bog, but most of this has been cut away. There are some outliers, and one long ridge, partly levelled and obliterated, may be traced along the Kingsland road as far as the outskirts of the town of Athenry, at one time the Anglo-Norman capital of the province of Connaught.

Having spent a number of years in this peculiar locality, I have often considered how by any known agency of tide or current these ridges and sand-hills could be shaped as they are. While some features could be accounted for by marine action, others could not be brought within range. How could the 'flow-tide' heap up two great parallel ridges within a stone's cast of each other? If it is within the function of the tides to accomplish this, I am afraid we don't quite understand the question of ways and means. By Hummel's theory we get over the difficulty. There is remarkable parallelism among the åsar of the Lake Mälar district. And if there were no such difficulty in the way of the marine theory, there is a rather formidable one as regards the heaping up of a ridge so high and steep-sided in proportion to width at top.

The eastern end of the more northern ridge has been cut away to make room for some of the buildings, and in this way is made a very good section, which, although partly obscured by a wall and by detritus at the base, affords the best view of the internal structure of an esker that I have anywhere seen. This section shows a curious alternation of sand and gravel beds with an occasional 'leaf' of clay, or rather lime-clay paste, but the dip is not quite so steep as the sides. Owing to the percolation of rain-water, there is matrix of calcareous matter which serves to bind the whole into a tolerably compact mass, which, however, readily yields to the pick-axe, and the loosened materials when screened, serve to mix with mortar for building. Over the sides and top there is, however, a deposit of aërial drift, in which the rabbits can work their way. Formerly, the peat closely surrounded both ridges. I have heard it stated that there is peat underneath the gravel. But I know that in the summer of 1897 a pump was sunk within two yards of the section

I have described, and to the depth of twenty-five feet, without any indication of peat. The boring passed through beds similar to those of the ridge, but horizontal, and of closer grain. No boulders were met with until the water-bearing stratum had been tapped, and then some large ones were encountered.

In the sand-hill group, the few openings I have seen showed less of the stratified arrangement, with a greater number of rounded pebbles, exemplifying, I think, the passage of the true esker into the morainic mounds, much acted upon by denudation.

Are we then to take it as a settled matter that the esker sea is all a myth?

I hardly think so. Even Geikie admits the probability of an epoch of depression, and mentions that "gravel beds with marine shells have been traced in Ireland up to a height of 1235 feet on Montpelier Hill." Again, how are we to account for the presence of large blocks of the red porphyritic granite of western Galway on the Slieve Bloom mountains? This granite, as Jukes remarks, "is easily recognisable inasmuch as it contains hornblende instead of mica, and has large crystals of pinkish felspar, and is therefore porphyritic." How could these erratics be borne to their present situation except on rafts of floating ice? It is known that blocks of stone will sometimes rise through the glacier to its surface. But in such cases the erratics do not rise above the level of their origin; they merely describe a plane of less incline than the upper surface of the glacier. It may be contended that the period of depression did not synchronize with the formation of the eskers, and if so, we have new difficulties to meet. It may be that the eskers were not all formed at one time or in one way, and that most of the theories apply to a certain extent, while no one theory yet propounded, is comprehensive enough to cover all the ground, or clear enough to explain all the circumstances. I have no intention and no ambition to attempt a new solution. But I have frequently been struck by what appears to me the marked resemblance between some eskers and certain phenomena in progress round the shores of Galway Bay.

In his essay on "The Arenaceous Rocks of Ireland" (*Proc. R. Soc. Dublin*, n.s., vol. v., p. 507, 1887), Kinahan describes a curious spit of conglomerate at Lisbellaw, Co. Fermanagh, which he believes to have been formed in Silurian times after the manner of the Chesil Bank. "In Lyme Bay the flow tide current runs from the westward of Portland Bill which acts as a groyne; Chesil Bank or Beach becomes coarser and larger as it is followed east, till it forms a massive heap of shingle to the west of the Bill; but eastward of the Bill, in Weymouth Bay, there are finer accumulations. In Silurian times similar forces were at work in the neighbourhood of Lisbellaw."

And similar forces, I should say, were at work in the 'esker' sea, as they are at the present day in Galway Bay, and must have been at work on some parts of the margin of the sea in every geological age. Mr Kinahan makes incidental reference to the ridge from Co. Wexford to the Saltees. Within three miles of the mouth of the Corrib River, on which Galway stands, there are examples of what I take leave to call Chesil Bank formation in progress at the present hour, some to the east and some to the west of the outlet.

East of the town, and separated from it by the inner bay known as Lough Atalia (crossed by the railway) is the promontory on which stands Renmore Military Barracks. Off this headland is Hare Island, a rather remarkable fragment of the boulder-clay drift which appears in cliffs just opposite and in other places around the Bay. At low water this island is connected with the mainland by a natural causeway, about half a mile in length, of so regular construction that it would, at first sight, appear almost as the work of man. At high tides this causeway or bar is covered under water deep enough to float a small schooner. At the land end it joins other ridges more of the 'harbour-bar' character, running to right and to left along shore, and cutting off lagoons from the Bay, the bars being above the reach of all but the highest tides. "Who made this road ?" I asked an old man residing in the hamlet hard by. "The tide," he answered; "the bank has grown out from the island, and is still growing." The island divides the flow-tide, and the shingle and sand are ridged up very much as Kinahan's theory lays down. More curious still is the great loop which the ridge makes at the island, forming a deep pond or loch of over an acre in extent, the surface of the enclosed water being, when I saw it, at ebb-tide on 24th May 1898, fully ten feet above the level of the surrounding waters. Near the famous Claddagh, a great bank of shingle has cut off many acres inshore, flooded only at high tides through a gap in the 'bar.' At the western end of this ridge there is also a fragment of boulder-clay. Again, about a mile to the west of the beautiful sea-side suburb, Salthill, is a conspicuous promontory of the boulder-clay known as Mount Gentian (now the Golf Links). Off this headland is another island fragment of the clay-drift; and this too has been joined to the shore by a long narrow causeway of shingle which stands clear of the highest tides, and may be traced for a mile in the direction of Salthill promenade, in the form of a 'harbour-bar,' cutting off a considerable space of old beach now converted into grazing ground..

The two marine causeways here described present some striking resemblance to many of the inland ridges I have seen. There is resemblance in contour, of slope, of proportion, of bedding; and if

the 'causeways' have a preponderance of pebble and shingle, that is owing to the circumstance that the shore in question is thickly strewn with granitic boulders.

It is not necessary, I think, to point out that I do not advance these remarks as a solution of the esker question. At the same time, I submit that the circumstances are deserving of some attention when the question is as to the way or ways in which the typical eskers have been formed. However much we may feel inclined to prefer the explanations offered by Hummel and Geikie, we cannot, I fancy, yet afford to discard in its entirety the principle so ably worked out by Kinahan. That there is a ridge-forming power in the flow-tide action of the great ocean—when the current meets some object sufficient to part it in the shallower waters near the coast—we can see for ourselves in such instances as I have just cited. That the eskers, properly so-called, are mainly confined to the zone which may be regarded as a continuation of Galway Bay is, to my mind, a circumstance more easily reconcilable with Kinahan's than with Geikie's later theory. Yet I do not regard this aspect of the question as by any means fatal to the principle propounded by the latter. If we have not typical eskers outside the midland zone we have at least their ruins, and these in plenty, in the counties of Monaghan, Tyrone, Fermanagh, Mayo, etc.; that is, we have such heaps, mounds, and flats as would be produced by the partial dispersion of a true esker ridge. In whatever way we may suppose the ridge to have been produced we can see that there were many chances against survival in the perfect form. That so many have withstood 'the shocks of time and chance' is rather wonderful; for hardly less curious or less puzzling than their origin is the question as to the conservation of so many eskers in Ireland and of corresponding ridges in other countries.

Thomas Fitzpatrick.

St Ignatius' College,
Galway.

IV

The Grey Mullet Fishery in Japan

THERE are three known species of *Mugil* in Japan, namely, *M. cephalotus*, *M. haematocheilus*, and *M. joyneri*.

The first species is known by the name of *Shirome* (white eyed), *Bora*, *Nayoshi*, &c., and while immature *Subashiri*, *Oboko*, *Ina*, &c., according to different stages of development. The second species is called *Akamĕ* (red eyed), *Shukuchi*, &c.; and the third species, *Meina*. The first species is abundant along the whole coast of the main island or Hondo, and is captured throughout the whole year. In summer we find innumerable fry of this fish in brackish water, and often in fresh water too. In spring the fish migrate in shoals towards shallow water, and remain scattered there during the whole summer, while in autumn they assemble together and form shoals again and migrate along the coast. Then as winter approaches they gradually seek warmer and deeper water, and pass the cold season in rather a dormant state. They are also cultured in brackish ponds, and sometimes in canals round such rice fields as are near the sea.. Their average length is a little more than a foot.

The second species, *Mugil haematocheilus*, is abundant in the southern district, and is caught in autumn when it comes in large shoals towards the coast for the purpose of spawning. Roes of this fish are salted and dried, and are highly esteemed as a delicacy. This fish attains a larger size (about two feet) than the preceding species.

The third species, *M. joyneri*, is not abundant, and consequently is not important economically.

The average annual catch of the grey mullet is about four hundred thousand *yen* in value, about £40,000. There are very diverse methods for its capture. Sweep-seines, dip-nets, pound-nets, hand-nets, stop-nets, set-nets, drift-nets, cast-nets, and drift-lines are the chief apparatus used for the purpose. These appliances are chiefly used in shallow waters, not more than ten fathoms in depth, as the fish are mostly found in these parts.

The mullet, especially *Mugil cephalotus*, is said to be frightened by sound and light. Moreover, it is said that the fish hate the presence of oily substances, so that they turn their route of migra-

tion from the place where such are found. Therefore, at the fishing ground, fishermen do not throw away any oily substance out of the boat; moreover, they keep very quiet, uttering no word.

The sweep seine.—There are many kinds of this seine, different in structure as well as in the method of using. I shall describe some nets of special interest.

The double floats seine (Fig. 1).—This is simply a long, narrow net, of which the height is greatest in the middle and decreases gradually towards either end. The length of the rope at both upper and lower margins of the net is 600 feet. This net has a very long and narrow netting added to its upper margin like a roof, to prevent the fish from leaping out of it. The narrow netting has its own floats on the free margin, hence the seine has two rows of floats. This special structure makes the net adapted for the capture of the mullet. When

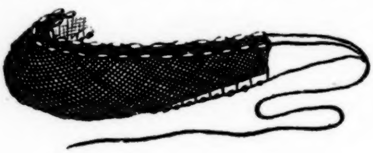

FIG. 1.

the net is in use, a small boat constantly attends to the roof-like netting to make it keep the right position and not to collapse; this is done by binding two or three floats together, as the seine is gradually drawn towards the shore and the two wings approach each other more and more. This seine is used by a boat manned by seven men to encircle a shoal of fish, and it is done under directions from a watch-tower on shore. When the seine is completely put out, it is dragged in by about thirty men on shore. This net can be used only in flat sandy shores.

The sweep-seine fishery with a sunken rope.—In certain parts of Central Japan a peculiar method is employed. During the night a big straw rope is sunk to the bottom of the sea at right angles to the coast line, and fishermen wait silently on land for the approach of fish with a boat ready in which a sweep seine is loaded. It is said that when a shoal of fish approaches, some fish are frightened by the sunken rope and leap out of water. The fishermen hearing the sound of the leaping fish, jump into the boat and hastily encircle the seine.

The dip net (Fig. 2).—Of this net also there are many kinds. It is used generally on rocky ground. It is rectangular or square in shape, and is mostly of a large size (some nets measure about 240 feet by 120 feet). The net is sunk at a convenient place in the route of migration of the fish. In some cases the net is

entirely sunk under the surface of water, while in other cases only one side is sunk. In rocky districts where the sea is generally deep, the mullet swims very close inshore. The net is often set in a bay having a slight curvative, because such a place is more convenient than an open coast or a coast deeply indented. In the former case the sea is more rough, while in the latter case the fish does not come often. The movement of the fish is observed from towers on land, and the communication between them is made by signals. When the fish come on the net, the sunken side or sides are immediately

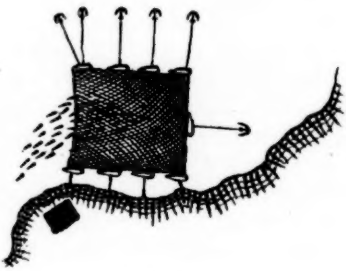

FIG. 2.

raised by hauling ropes which are fastened to the sides of the net. Ropes are held by anchored boats, and are hauled by a signal from the watch-tower nearest to the net. Such a dip-net is sometimes accompanied with two long wings, which are used to encircle the fish and force them to go on to the sunken net as well as to prevent them from escaping. As the quick performance of work is necessary for this fishery, numerous boats and men are generally employed.

The pound-net.—The apparatus (Fig. 3) next to be described is not a proper pound-net, because fish do not come into the net by themselves, moreover there is no special device to prevent the

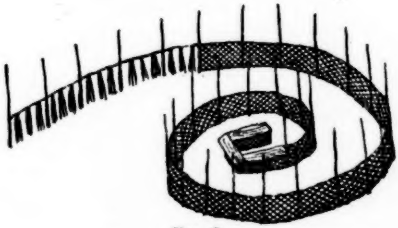

FIG. 3.

fish which enter the net from going out again. The net is spiral in shape. It is supported by bamboo sticks, stuck into ground, and consists of three different parts—outer, middle, and inner or central.

A straw rope with a few stems of straw, put between strands of the rope and suspended at regular intervals, forms the outer part. The end of these straw stems nearly touch the ground. The middle part forms the greater portion of the pound-net, and is made of a netting which is stretched between bamboo sticks to form a barrier. The central part is short. Its essential portion consists of three wooden boards, placed in the shape of the letter U. The space between the boards and the ground is cautiously closed by a netting. To catch fish, three or four persons row a small boat very swiftly, and at the same time drive scattered fish by beating the surface of water with oars or poles. When the boat comes near the end of the net, whence it cannot go further, a man comes out of the boat and continues the driving in the same way, wading towards the centre of the net. The fish are at last compelled to leap to clear the barrier, and are caught by the boards. This net is used in a shallow ground at the outlet of a brackish lake. The mouth of the net faces the outlet of the lake.

There is another kind of pound-net (fig. 4) used in a shallow bay where there is a great difference in the height of water between the flood and ebb tides. It is a very long net, set by means of poles, parallel to the coast line, and bent towards the coast at both ends. There is a fold, or a series of pockets, which runs through the whole

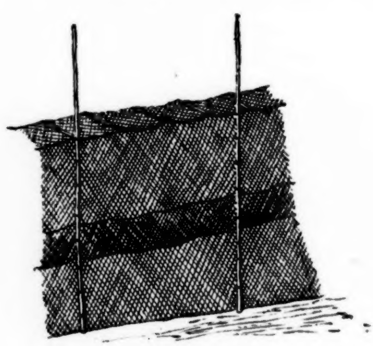

FIG. 4.

length of the net at its middle part. The mouth of the pocket opens towards the coast. When the tide recedes, the upper part of the net is bent backward to form a roof. The roof is supported by a series of floats. Fish are entangled in the pockets, or caught on the roof-like part.

The hand net (fig. 5).—This is a simple apparatus, but the method of using it is somewhat interesting. It is trapezoid in shape; its upper and lower margins are strengthened by ropes, and the two slanting sides by bamboo poles. This net is used on the shallow

sandy shore of a bay. The two bamboo sticks are held by two men wading in the water, or by two boats. Thus the net is expanded between them, special care being taken not to leave an open space between the lower margin of the net and the ground. The net is held obliquely against current. While the net is thus prepared, two boats row swiftly and drive fish towards the net by means of a long

FIG. 5.

scare-cord held between them. When the cord touches the net, the latter is quickly raised out of water to bail out the fish. The scare-cord is made of hemp, and is about 900 ft. in length. It is provided with numerous thin, small, rectangular pieces of wood (ea. 1 ft. by 2 ins.). They are slightly curved, and make a noise, and disturb the surface of the water very much when they are drawn quickly.

The stop net.—This is chiefly used in harbours to stop escape of fish, either by encircling a shoal of fish, or by closing the mouth of the harbour. This is generally accomplished with set-nets or gill-nets. For the benefit of this fishery, a certain district is closed

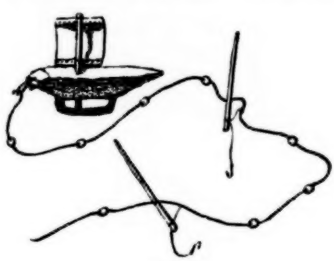

FIG. 6.

to fishermen, and sometimes even to the anchorage of boats, during a certain season. To allure fish to such a place, bran and mud are mixed together and made into balls, which are distributed over the ground.

The drift line (fig. 6).—This is used from a boat in a brackish lake. The line is made of hairs from horse tails, twisted together

to the thickness of about one line, and 120 to 140 yards in total length. Hooks, about ten in number, are attached to the line by means of short snoods. Moreover, there are many small, round floats attached to the line. The snood is provided with a long float made of wood near the point of attachment of the snood to the line. When a fish is caught on a hook, the long float belonging to the hook stands out of the surface of water. The hooks are baited with earthworms. At the distal end of the line a boat-shaped float with a sail is tied, by means of which the line is sent far from the boat. This apparatus is used only for sport. K. Kishinouye.

Imperial Fisheries Bureau,
Tokio, Japan.

V

The Zoological Congress

THE fourth meeting of the International Zoological Congress was held at Cambridge between the 22nd and 27th of August, under the presidency of Sir John Lubbock. The attendance was more than twice as great as at any previous meeting, and the whole Congress must be regarded as an unqualified success.

The plan of work comprised a discussion on some problem of general zoological interest in the morning, a series of sectional meetings for the consideration of more technical matters in the afternoon, with garden-party or reception in the evening.

The proceedings opened on Monday evening with a reception by the Mayor of Cambridge to the members of the two Congresses of Zoology and Physiology, both of which were sitting during the same week. On Tuesday morning Sir John Lubbock began the work of the Congress by delivering his Presidential Address. Following the precedents set by the previous Presidents, Milne Edwards, Kapnist and Jentink, Sir John Lubbock confined his address to a welcome to the members, some interesting remarks on the great field of work still open to zoologists, and reference to the flourishing school of zoology which has its seat at Cambridge. The general report of the Congress was read by the treasurer, and Dr van Hoek announced some reforms in the postal charges for natural history specimens that were to be granted. Zoological nomenclature was brought up in reference to a report of a committee on that eternal question; but, to the general relief, any discussion was cleverly shelved until the next Congress.

The two principal meetings of the Congress were held on Wednesday and Thursday morning. At the former, Professor Delages and Mr Minchin opened a discussion on the "Position of Sponges in the Animal Kingdom." The fight soon resolved itself into a long-range skirmish between the old men and the young. Delages and Minchin both held that the inversion of the embryonic layers in the sponge precludes the reference of that group to the Coelenterata, though whether it is to be ranked as a separate group, or as directly descended from the Choanoflagellata seemed less certain. Haeckel and Schulze both clung to the Coelenterate theory. Vossmaer declined to express an opinion, and Saville Kent reaffirmed his belief in the

affinity of the sponges with Choanoflagellata. The general trend of the discussion certainly seemed to favour the newer view.

On Wednesday morning the problem for discussion was the Origin of the Mammalia, opened by Professors Seeley and Osborn, and continued by Marsh, Sedgwick, Hubrecht, and Haeckel. The main question round which the discussion ranged was whether the mammals had descended from the reptiles, instead of from the amphibia, as is now generally taught in text-books in accordance with Huxley's teaching. Professor Seeley's speech was a clear comparison of the osteology of the anomodont reptiles with that of mammals. He showed that all the supposed mammalian characters are also found among the extinct anomodonts, which have completely broken down the distinction between reptiles and mammals. Professor Osborn began by deducing the probable characters of the ancestral mammal from certain general considerations. He agreed with Seeley as to the origin of mammals from reptiles, but preferred to regard the former as descended from a third, as yet undiscovered, group of anomodonts, instead of going back to the Devonian or Silurian for some common ancestor of mammals and anomodonts. Marsh expressed no positive opinion on the main question, but insisted on the fact that the reptiles and mammals are still separated by four important osteological characters. He adduced strong reasons for considering that the resemblances between mammals and reptiles adduced by Professor Seeley may be explained as a case of parallel adaptation, the same characters having been independently acquired. Sedgwick made an effective speech protesting that embryological evidence will not help in the solution of such a remote question as that under discussion, as it gives no indication of the polydactyle stage of the ancestral horse, the toothed stage of birds, or the limbed stage of snakes, though the existence of those stages is not doubted by any one. Professor Hubrecht did his best to defend embryology from this emphatic statement of its limitations, and Haeckel, strangely conservative, still upheld the origin of all placental mammals from the marsupials. But this view was generally scouted, and the discussion seemed to strengthen the case for the reptilian ancestry of the mammals.

The principal feature in the Friday morning's proceedings was Haeckel's discourse on the Descent of Man. The previous day's discussion anticipated much that he intended to say, so he did not read his paper, and until that is printed it is not possible to estimate the scientific value of his contribution. His speech was popular, and Haeckel received an ovation at its close.

The afternoon meetings were devoted to papers of more technical character and exhibitions in the museum. The papers were of very unequal value. A few were advertisements of forthcoming works,

by the exhibition of sample plates. That comparatively little of the time of the Congress was wasted in this way is no doubt due to the courage with which the secretaries had rejected the more obviously useless papers. Among the exhibits those of Mr Graham Kerr, Mr Stanley Gardiner, Mr Willey, and Mr Rousselet are especially worthy of mention.

On Saturday morning a business meeting was held, at which it was arranged that the next meeting should be in Germany during 1901. The Congress then adjourned to London for visits to the Zoological Gardens and British Museum (Natural History), where on the Saturday evening Sir John Lubbock gave a reception. On Monday a large number of members went to Tring to see Mr Rothschild's magnificent museum, and on Tuesday a smaller party under the guidance of Mr Lydekker inspected the Duke of Bedford's collection of deer at Woburn Abbey.

In such a varied course of proceedings it is difficult to draw any general conclusions as to results. Thanks to the skilful plans of the secretaries, Professor Jeffrey Bell, Mr Sedgwick, and Mr Bourne, and the tactful arrangements of the secretaries of the local reception committee, Dr Harmer and Mr Shipley, the Congress was held without a hitch. The opinion was generally expressed that the scientific standard of the papers was much higher than usual. The discussions were especially enjoyed, and will no doubt henceforth be regarded as a chief feature of future meetings. From the proceedings three impressions seem to have been generally felt. First, the scanty attendance of entomologists and ornithologists may indicate the increasing separation of those branches of zoological work. The ornithologists have a congress to themselves, and the entomologists also will not improbably start one, to settle their own difficulties without the interference of specialists in other subjects. Secondly, the greater importance attached to palaeontology, and the increasing distrust of embryology as a guide in phylogeny, were shown repeatedly. Thirdly, there seemed a feeling of boredom with the interminable question of zoological nomenclature, and a certain determination to refuse to follow rigid rules when they lead to absurdities, and to trust more in the future to common sense.

VI

James Hall

IT has been given to few men to serve for upwards of sixty years on the official staff of a public department in the Old World; and probably the late Dr Hall's sixty-two years of service in the Geological Survey of New York State is unique in American annals.

James Hall was born on the 12th September 1811, at Hingham, an old-fashioned New England town near Boston, which claims the oldest occupied church in America. Early in the century it was little more than a fishing village, and Hall received his scientific education in the Rensselaer School at Troy, which has since grown into the Troy Polytechnic Institute. He graduated there in 1832, when he was appointed assistant professor of chemistry and natural science. In 1836 he was transferred to the professorship of geology, and in the same year was appointed to one of the posts of assistant geologist on the newly established Geological Survey of New York State. In the following year he was promoted to the rank of State Geologist, and began field work during the same year. From 1838 until 1843 he was engaged on the survey of the western part of the state, and began the study of the recession of the Niagara Falls, to which he acted as Lyell's guide in 1841. In 1843 he wrote his last field report, and was appointed the State Palaeontologist. Under his supervision systematic fossil collecting was undertaken in the rich palaeozoic faunas of New York, resulting in the formation of the magnificent collections in the Albany Museum. The faunas were described in "The Palaeontology of New York," of which thirteen huge imperial quarto volumes were issued between 1847 and 1894 at the estimated cost of about a million dollars. In addition to writing and editing this great work, Hall contributed a long series of reports to the annual volumes of the "*Regents Reports of the New York State Museum*," and a number of other papers in the usual scientific serials. In 1848 he was elected a Foreign Member of the Geological Society, and ten years later received from the same society its Wollaston Medal, of which at his death he was the senior recipient by no less than eleven years, while the third medallist in seniority was elected twenty-three years later. In 1855 he was offered the post of palaeontologist to the Canadian Geological Survey, with a promise of the reversion of Sir William Logan's position of Director. He declined the offer, but worked out the collection of Canadian graptolites, which were described in a report on "The Graptolites of the Quebec Group," published in 1865.

In 1855 Hall accepted the post of State Geologist of Iowa.

That office and the similar position for the state of Wisconsin, which Hall received in 1857, he was able to hold without affecting his position in the New York service. His work in connection with these surveys increased his knowledge of the geology of the Mississippi basin; and he was able to make important additions to the geology of the western states, as he was entrusted by the Federal Government with the description of the collections made by many of the expeditions and surveys in that region. Thus he described the fossils collected by the Fremont expedition, the collections of the Pacific Railway Survey, the Cretaceous fossils obtained by the Mexican Boundary Commission, and wrote a report on the geology and palaeontology of the basin of the Great Salt Lake of Utah from materials brought back by Lieutenant Stansbury.

In 1856 Hall was elected President of the American Association for the Advancement of Science. At its meeting in Montreal in the following year he gave his famous address, in which was first definitely expounded the theory that the elevation of mountain chains is due to the deposition of sedimentary deposits, and that the direction of the mountain chains are determined by lines along which the thickest accumulation has taken place.

But this and similar incursions into the domain of physical geology only temporarily distracted Hall from palaeontographical work to which his inexhaustible energies were mainly devoted. His additions to the materials of invertebrate palaeontology are probably greater than those of any other man. The number of important new genera founded by Hall is enormous. The roll of Hall's new genera and species was by 1858 so long, that Colonel Portlock, the President of the Geological Society whose duty it was to present Hall with the Wollaston Medal, felt bound to qualify his patronizing commendations by a warning that he was himself "prone to hesitate respecting new species when closely allied to previously known species." But the work which Colonel Portlock appeared to disparage is now recognised as Hall's most permanent title to fame. As a note in the *Geological Magazine* reminds us, palaeontology is indebted to Hall for the following important genera: among the graptolites there are *Callograptus*, *Dicranograptus* and *Phyllograptus*; among the corals, *Coelophyllum*, *Heliophyllum* and *Streptelasma*; among the Pelmatozoa, *Calceocrinus*, *Heterocrinus*, *Dendrocrinus*, *Glyptaster*, *Glyptocrinus* and *Hemicystis*; there is the star-fish *Palaeaster*, and the echinid *Lepidechinus*; the additions to the Monticuliporoids and Bryozoa are very numerous, including *Favistella*, *Callopora*, *Bactropora*, and *Trematopora*; and among the Trilobites are *Pleuronotus*, *Bathynotus*, *Mesothyra* and *Ptychaspis*. His Memoir on North American *Eurypterida*, *Pterygoti* and *Ceratiocaris* (1871),

is one of the most valuable contributions to these forms of Arthropods. And Hall not only described fossils systematically; he carefully studied their anatomy and the microscopic structure of their tissues, a work in which the magnificent preservation of the North American palaeozoic fossils gave him exceptional opportunities.

In 1872 Hall visited England to attend the meeting of the British Association at Brighton, and read a paper "On the Clinton, Niagara, and Upper Helderberg Formations in the United States." Four years later he helped to found the International Congress of Geologists, an institution in which he always took a keen interest. In fact only last year, in spite of his 86 years of age, he visited Russia to attend the last meeting of the Congress and subsequently accompanied it in a fatiguing excursion through the Ural Mountains. In 1884 Hall was elected a corresponding member of the Paris Academy of Sciences.

During the last few years of his life Hall was greatly worried by the friction with the literary departments of the New York State service with which his own was associated. Originally the scientific departments were under the control of the library and literary branches of the service, which regarded the great cost of the scientific departments with disfavour. At length in 1893 Hall succeeded in getting a bill through the New York legislature, which secured his freedom from literary control. An attack was then made on his private character. Mr Melvil Dewey, the state librarian, charged Hall with having sold for 65,000 dollars a collection of fossils which he said were really the property of the State. A legislative committee investigated the charges, Dr Hall was triumphantly acquitted, and the New York Geological Survey has continued its work in peace.

Hall's indomitable energy, unfailing courtesy and bright good humour rendered him an universal favourite in American scientific circles. His humour was keen, and though sometimes cynical, never marred by any suspicion of unkindness. For example, his wife was a Roman Catholic and had at one time converted Hall to her views. Hall used to attribute his abandonment of Romanism to the breaking of a pulley chain, whereby there were simultaneously smashed to shivers one of his favourite fossils and his faith in providence. After this Hall's relations with his wife were not so sympathetic; for she appeared a little jealous of his devotion to his scientific work. So he built a house for his wife in his park, and they lived together for years on terms of friendly neighbourship. She died some years ago, and Hall keenly felt her loss. He himself passed away rather suddenly on August 7th, at a quiet resort in the White Mountains, where he had gone for his usual summer's rest.

VII

Animal Intelligence as an Experimental Study

THE investigation of the problems suggested by the observable phenomena of instinct and intelligence in animals is passing—we may now say has passed,—into the experimental stage. The collection of anecdotes, useful enough for preparing the ground and (as Time's irony has shown) for enabling one to perceive the insecurity of any such basis for reliable conclusions, has had its day. It is realized by serious students that, not only for the interpretation but also for the observation of the phenomena, if they are to serve the ends of science, some real training and discipline in psychology are essential. Dog-stories and cat-stories though often full of subtle humour and though not infrequently revealing an affectionate and imaginative nature, serve rather to tickle the fancy than to appeal to the rational faculties. It is not on such foundations, nor with such materials, that a science of comparative psychology can be securely built. Observations *ad hoc* by an investigator trained *ad hoc*, will always carry weight. But the casual jottings of well meaning though uninstructed people serve rather to check than to forward the diffusion of exact knowledge.

Mr E. L. Thorndike in a monograph on "Animal Intelligence" published as a supplement to the *Psychological Review* (June 1898) has approached his subject in the right way, as one full of difficult problems to be grasped, faced, and if possible solved, and has furnished an experimental basis, narrow perhaps, but capable of further extension for the conclusions that he draws. I have briefly noticed his work elsewhere (*Nature*, July 14th, 1898); but I regard it as of sufficient importance to justify a more extended presentation and consideration here.

The subjects (one might, alas! almost say victims) of Mr Thorndike's experiments—or those to which the exigences of space compel us to confine our attention—were thirteen kittens or cats from three to eighteen months old. His method of investigation shall be stated in his own words.

"After considerable preliminary observation of animals' behaviour under various conditions, I chose for my general method one which, simple as it is, possesses several other marked advantages besides those which accompany experiment of any sort. It was merely to put animals when hungry in enclosures from which they could escape by some simple act, such as pulling at a loop of cord,

pressing a lever, or stepping on a platform. The animal was put in the enclosure, food was left outside in sight, and his actions observed. Besides recording his general behaviour, special notice was taken of how he succeeded in doing the necessary act (in case he did succeed), and a record was kept of the time that he was in the box before performing the successful pull, or clawing, or bite. This was repeated until the animal had formed a perfect association between the sense-impression of the interior of that box and the impulse leading to the successful movement. When the association was thus perfect, the time taken to escape was, of course, practically constant and very short.

"If, on the other hand, after a certain time the animal did not succeed, he was taken out, but *not fed*. If, after a sufficient number of trials, he failed to get out, the case was recorded as one of complete failure. Enough different sorts of methods of escape were tried to make it fairly sure that association in general, not association of a particular sort of impulse, was being studied. Enough animals were taken with each box or pen to make it sure that the results were not due to individual peculiarities. None of the animals used had any previous acquaintance with any of the mechanical contrivances by which the doors were opened. So far as possible the animals were kept in a uniform state of hunger, which was practically utter hunger."

To Mr Thorndike's monograph we must refer those who desire detailed information as to apparatus and procedure. It must here suffice to state that the box-cages employed were rudely constructed of wooden laths, and formed cramped prisons about twenty inches long by fifteen broad and twelve high. Nine contained such simple mechanisms as Mr Thorndike describes in the passage above quoted. When a loop or cord was pulled, a button turned, or a lever depressed, the door fell open. In another, pressure on the door as well as depression of a thumb-latch was required. In one cage two simple acts on the part of the kitten were necessary, pulling a cord and pushing aside a piece of board ; and in yet others three acts were requisite. In those boxes from which escape was more difficult a few of the cats failed to get out. The times occupied in thoroughly learning the trick of the box by those who were successful are plotted in a series of curves, the essential feature of which is the graphic expression of a gradual diminution in the time interval between imprisonment and escape in successive trials. In some cases the cats were set free from a box when they (1) licked themselves or (2) scratched themselves.

Mr Thorndike comments on the results of his experiments as follows :—

"When put into the box the cat would show evident signs of discomfort and of an impulse to escape from confinement. It tries to squeeze through any opening ; it claws and bites at the bars or wire ; it thrusts its paws out through any opening and claws at everything it reaches ; it continues its efforts when it strikes anything loose and shaky : it may claw at things within the box. It does not pay very much attention to the food outside, but seems simply to strive instinctively to escape from confinement. The vigour with which it struggles is extraordinary. For eight or ten minutes it will claw and bite and squeeze incessantly. . . . The cat that is clawing all over the box in her impulsive struggle

will probably claw the string or loop or button so as to open the door. And gradually all the other non-successful impulses will be stamped out and the particular impulse leading to the successful act will be stamped in by the resulting pleasure, until, after many trials, the cat will, when put in the box, immediately claw the button or loop in a definite way. . . . Starting, then, with its store of instinctive impulses, the cat hits upon the successful movement, and gradually associates it with the sense-impression of the interior of the box until the connection is perfect, so that it performs the act as soon as confronted with the sense-impression. . . . Previous experience makes a difference in the quickness with which the cat forms the associations. After getting out of six or eight boxes by different sorts of acts the cat's general tendency to claw at loose objects within the box is strengthened and its tendency to squeeze through holes and bite bars is weakened; accordingly it will learn associations along the general line of the old more quickly. Associations between licking or scratching and escape are similarly established, and there was a noticeable tendency to diminish the act until it becomes a mere vestige of a lick or scratch. After the cat gets so that it performs the act soon after being put in, it begins to do it less and less vigorously. The licking degenerates into a mere quick turn of the head with one or two motions up and down with tongue extended. Instead of a hearty scratch, the cat waves its paw up and down rapidly for an instant."

These experiments confirm the conclusion to which I have been led by my own observations that the method of animal intelligence is to profit by chance success and to build upon fortunate items of experience casually hit upon and not foreseen. I need not here repeat cases already published, such as the opening of a gate on the part of my fox terrier by lifting the latch, a trick he certainly learnt by this method; but I may very briefly describe one or two further observations not yet recorded. I have watched my dog's behaviour when a solid indiarubber ball was thrown towards a wall standing at right angles to its course. At first he followed it right up to the wall and then back as it rebounded. So long as it travelled with such velocity as to be only just ahead of him he pursued the same course. But when it was thrown more violently, so as to meet him on the rebound as he ran towards the wall, he learnt that he was thus able to seize it as it came towards him. And, profiting by the incidental experience thus gained, he acquired the habit—though for long with some uncertainty of reaction—by slowing off when the object of his pursuit reached the wall so as to wait its rebound. Again, when the ball was thrown so as to rebound at a wide angle from a surface, at first,—when the velocity was such as to keep it just ahead of him,—he followed its course. But when the velocity was increased he learnt to take a short cut along the third side of a triangle, so as to catch the object at some distance from the wall. A third series of experiments were made where an angle was formed by the meeting of two surfaces at right angles. One side of the angle, the left, was dealt with for a day or two. At first the ball was directly followed. Then a short cut was taken to meet its deflected course. On the fourth day this method was well estab-

lished. On the fifth the ball was thrown so as to strike the other or right side of the angle and thus be deflected in the opposite direction. The dog followed the old course (the short cut to the left) and was completely non-plussed, searching that side and not finding the ball for eleven minutes. On repeating the experiment thrice similar results were that day obtained. On the following day the ball was thrown just ahead of him so as to strike to the right of the angle and was followed and caught. This course was pursued for three days, and he then learnt to take a short cut to the right. On the next day the ball was sent, as at first, to the left and the dog was again non-plussed. I have not yet succeeded in getting him to associate a given difference of initial direction with a resultant difference of deflection. And since these words were written the dear little fellow has died. No doubt it will be said by some fortunate possessor of a particularly rational dog that my fox terrier was a fool. Let him experiment and record the stages of progress, remembering that a rational being will quickly and surely pierce to the heart of the mystery.

I may here mention that whenever searching for a ball of which he had lost sight in the road he would run along the gutter first on one side and then on the other. A friend who was walking with me one day regarded this as a clear case of rational inference. "The dog knows," he said, "the effects of the convex curvature of the road as well as we do." I am convinced, however (having watched his ways from a puppy), that this method of search was gradually established on a basis of practical experience. No logical inference on his part is necessary for the interpretation of the facts; and we should not assume its presence unless the evidence compels us to do so.

Such experiments carried out on a different method give results in line with Mr Thorndike's. The conditions are more natural which I regard as in some respects an advantage. But we need experiments on different methods,—the more the better,—and if the results they furnish are in accord, their correctness will be rendered the more probable. I hope, however, that Mr Thorndike will devise further experiments in which (1) the conditions shall be somewhat less strained and straitened, while the subjects are in a more normal state of equanimity (cannot "utter hunger" be avoided?), and (2) there shall be more opportunity for the exercise of rational judgment, supposing the faculty to exist. To establish the absence of foresight in the procedure of the cats, it is surely necessary so to arrange matters that the connections are clearly open—nay even obvious—to the eye of reason. It appears to me that this consideration has not weighed sufficiently with Mr Thorndike.

A number of interesting experiments were made with a view to testing the influence, if any, of imitation.

"A box was arranged with two compartments separated by a wire screen. The larger of these had a front of wooden bars with a door which fell open when a string stretched across the top was bitten or clawed down. The smaller was closed by boards on three sides and by the wire screen on the fourth. Through the screen a cat within could see the one to be imitated pull the string, go out through the door thus opened and eat the fish outside. When put in this compartment, the top being covered by a large box, a cat soon gave up efforts to claw through the screen, quieted down and watched more or less the proceedings going on in the other compartment. Thus this apparatus could be used to test the power of imitation. A cat who had no experience with the means of escape from the large compartment was put in the closed one; another cat, who would do it readily, was allowed to go through the performance of pulling the string, going out, and eating the fish. Record was made of the number of times he did so and of the number of times the imitator had his eyes clearly fixed on him. . . . After the imitatee had done the thing a number of times, the other was put in the big compartment alone, and the time it took him before pulling the string was noted and his general behaviour closely observed. If he failed in 5 or 10 or 15 minutes to do so, he was released and not fed. This entire experiment was repeated a number of times. From the times taken by the imitator to escape and from observation of the way that he did it, we can decide whether imitation played any part. . . . No one, I am sure, who had seen the behaviour of the cats would have claimed that their conduct was at all influenced by what they had seen. When they did hit the string the act looked just like the accidental success of the ordinary association experiment. But, besides these personal observations, we have in the impersonal time-records sufficient proofs of the absence of imitation. It therefore seems sure that we should give up imitation as an *a priori* explanation of any novel intelligent performance. To say that a dog who opens a gate, for instance, need not have reasoned it out if he had seen another dog do the same thing, is to offer instead of one false explanation another equally false. Imitation in any form is too doubtful a factor to be presupposed without evidence."

Mr Thorndike is of opinion that monkeys are probably imitative in a sense that cats and dogs are not. But this is not at present substantiated by analogous experiments. I trust that he will submit it to this test.

As Mr Thorndike himself well observes, it is necessary clearly to differentiate the various meanings which are intended when the word "imitation" is used. The most elementary form of imitation—that, of which, I believe, we find abundant evidence in the procedure of animals—is where the performance of a simple act by one individual suggests the performance of a similar act by another. This is the "plastic limitation" of Professor Mark Baldwin, and is analogous to mimicry as a biological phenomenon in this respect; it is imitative from the observer's point of view but does not imply intentional imitation on the part of the performer. Conscious and purposive imitation involves faculties of a high order; and I am not prepared to accept its existence in animals,

even the Primates, without more conclusive evidence than is at present forthcoming. But unconscious imitation of the follow-my-leader type (the outcome of a direct suggestion) is a factor of prime importance alike in animal and in human life. And of this Mr Thorndike's experiments do not offer any disproof. A cat with no experience of the means of escape sees another perform a certain act and learns nothing from the experience. This no doubt proves that the cat had not in any sense grasped the problem to be put before it; and shows that when placed in similar difficulties it did not go back upon its previous merely observational experience (if such it can be called). But the previous experiments have already gone far to disprove the rationality of the cat—have at any rate thrown the onus of proof on the upholders of the alternative hypothesis. The whole gist of the chance experience interpretation of animal behaviour is that there must be such chance experience to build on. The cat cannot gain this by looking on, never so intently, unless it be provided with a rational, as well as a sensory eye. But the act of pulling the string is not of the type that can reasonably be regarded as likely to afford a follow-my-leader suggestion. It has been reached by the gradual elimination of many failures; it is a differentiated act, and one therefore so far removed from the ordinary procedure of kittens. In all this I think Mr Thorndike will agree. But his statements might well lead readers of his work to suppose that he denied this influence of suggestion. When he lays it down that "to say that a dog who opens a gate, for instance, need not have reasoned it out if he had seen another dog do the same thing, is to offer instead of one false explanation another equally false," he is, I think, open to misconstruction. Puppies at a gate do most certainly in some cases (I speak from observation) follow the lead in an unmistakable manner, and unquestionably profit by the suggestive behaviour of one of their number. To contend that they imitate with conscious intent would be quite another matter.

A series of experiments were made to ascertain whether instruction (in the form of putting the animal through the procedure requisite for a given act) was in any degree helpful. The conclusion is that such instruction has no influence. Those who have had experience in teaching animals to perform tricks will probably agree here—though some trainers give expression to a different opinion. It is, however, essential carefully to distinguish between showing an animal how a trick is done, and furnishing useful accessory stimuli (such as the occasional taps of the trainer's whip) which temporarily enter into the association complex. If the latter be eliminated the practice of trainers, I believe, bears out the general result of the experiments. Mr Thorndike never

succeeded in getting an animal to change its way of doing a thing for his. Nor was I, after repeated trials, able to modify the way in which my dog lifted the latch of the gate. He did it with the back of his head. I could not get him to do it (more gracefully) with his muzzle.

It is not my purpose to discuss Mr Thorndike's psychological analysis of the procedure of the cats. I think he is a little disposed to emphasise the points in which he and I differ—though in many cases the difference is more apparent than real. But in truth we agree on more points than I care here to enumerate. I fully concur in the opinion that what we have to deal with in animal intelligence is a sequence of conscious situations; that it is only through analysis of mental complexes that we separate and isolate free ideas; that man can do this, and that the animal in all probability can not. I am absolutely at one with him in the belief that the method of animal intelligence to profit by chance experience without rational foresight, and that unless such experience be individually acquired, the data essential for intelligent progress are absent. While in our attempts to realise the general nature of animal consciousness there is a close similarity of treatment. In my *Introduction to Comparative Psychology* I devoted a good deal of space to an analysis of the psychology of skill "in order that we may infer what takes place in the minds of animals"; and I said:—"When I am playing a hard game of tennis, or when I am sailing a yacht close to the wind in a choppy sea, self does not at all tend to become focal. Hence, though I am a self-conscious being I am not always self-conscious. And presumably when I am least self-conscious, I am nearest the condition of the animal at the stage of mere sense experience. I am exhilarated with the sense of pleasurable existence, my whole being tingles with sentient life. I sense, or am aware of, my own life and consciousness, in an unusually subtle manner. Experience is vivid and continuous. Such I take it to be the condition of the conscious but not yet self-conscious animal."

I can therefore cordially endorse Mr Thorndike's conclusions as expressed in the following passages:

"One who has watched the life of a cat or dog for a month or more under test conditions, gets, or fancies he gets, a fairly definite idea of what the intellectual life of a cat or dog feels like. It is most like what we feel when consciousness contains little thought about anything, when we feel the sense-impressions in their first intention, so to speak, when we feel our own body, and the impulses we give to it. Sometimes one gets this animal consciousness while in swimming, for example. One feels the water, the sky, the birds above, but with no thoughts about them or memories of how they

looked at other times, or æsthetic judgments about their beauty; one feels no ideas about what movements he will make, but feels himself make them, feels his body throughout. Self-consciousness dies away. Social consciousness dies away. The meanings, and values, and connections of things die away. One feels sense-impressions, has impulses, feels the movements he makes; that is all."

And after an illustration from such a game as tennis, Mr Thorndike adds :—" Finally, the elements of the associations are not isolated. No tennis-player's stream of thought is filled with free-floating representations of any of the tens of thousands of sense-impressions or movements he has seen and made on the tennis-court. Yet there is consciousness enough at the time, keen consciousness of the sense-impressions, impulses, feelings of one's bodily acts. So with the animals. There is consciousness enough, but of this kind."

There is much in Mr Thorndike's monograph to which there is not space to allude. He is weak in that historical sense which gives continuity to the development of scientific interpretation, but I regard his investigation as one of great promise, and believe that its further prosecution will lead to other results not less important than those which he here presents. Experimental work in this field is sorely needed; and Mr Thorndike has proved himself one who is able and willing to carry it out.

C. Lloyd Morgan.

16 Canynge Road,
Clifton, Bristol.

SOME NEW BOOKS

Fossil Plants for Students of Botany and Geology. By A. C. Seward, M.A., F.G.S. (Cambridge Natural Science Manuals.) Pp. i-xviii+1-452. With frontispiece and 111 figures. Cambridge: University Press, 1898. Price 12s.

No better indication could be given of the increased interest in the study of fossil plants than that afforded by the publication during the last few years of several text-books specially dealing with this branch of Natural History. At last it seems to be recognised that the student of recent botany must possess some knowledge of fossil botany to enable him to have a comprehensive view of his subject and equally, if not even more important is it, for the palaeobotanist to be thoroughly conversant with recent botany and especially with those groups which are more closely connected with those occurring in the fossil state. It is only when Fossil Botany has been so studied that any real advance can be made, and the present work admirably brings before us in a clear and lucid manner, the results which have been attained within the last few years from the study of fossil botany on these lines. I am certainly not one to declaim against the older workers who laboured under disadvantages which the modern student can scarcely appreciate, and who by patient work laid the foundation on which all workers must build, but by modern methods of research unknown to them, fossil botany now holds an important position in biological science, to which it has only attained within the last few years.

Only the first volume of Mr Seward's work has appeared. It begins with the lowest forms known as fossils and ends with the Sphenophyllales. Part I., consisting of six chapters, deals with matters connected generally with the study of fossil plants, while Part II., comprising chapters vii.-xi., treats of the systematic portion of the subject.

Chapter i. gives a short historical sketch, while chapter ii. deals with the Relation of Palaeobotany to Botany and Geology. A short but very concise geological history is given in chapter iii., which is quite sufficient to show the succession and chief characteristics of the various "Formations." Chapter iv., which describes various modes of Preservation of Plants as Fossils, we consider one of the most important parts in the work. It is only after much practical experience in collecting and examining fossils, that one learns how much to allow for differences in appearance, even in the same species, which are entirely due to different modes of fossilization.

Chapter v. on the "Difficulties and Sources of Error in the Determination of Fossil Plants" is also admirably written and must be carefully read by all students. It illustrates the utter absurdity of classification based on external similarities of appearance or form. I may quote one sentence which shows a good use such errors may serve in the future. "It would serve no useful purpose, and I would occupy no inconsiderable space to refer at length to the numerous mistakes which have been committed by experienced writers on the

subject of fossil plants. Laymen might find in such a list of blunders a mere comedy of errors, but the palaeobotanist must see in them serious warnings against dogmatic conclusions or expressions of opinion on imperfect data and insufficient evidence"; and it is in this spirit, not one of severe criticism, that we must study many of the earlier writers.

Nomenclature is dealt with in chapter vi., and though a text-book is not the place to treat fully this "difficult and thorny question," the general principles are laid down for the protection of the name of the original describer as the author of the species, even though circumstances demand its removal to another genus than that in which it was originally placed.

Part II., beginning at chapter vii., deals with the systematic portion of the subject. The divisions of the plant kingdom are taken in their natural sequence, beginning with the lowest and passing gradually to the highest group. The Thallophyta are therefore first considered, the various groups or genera being preceded by a short account of their recent representatives. Here are included the *Girvanella* of Nicholson and Etheridge. The supposed fossil bacilli are described in some detail, and though some minute bodies are possibly correctly included here, a great deal of uncertainty hangs over many of these so-called Bacteria.

The Algae form a difficult class. Undoubtedly most of the fossils originally described as Algae are inorganic markings and animal tracks, though a few true Algae have been found in the fossil state—even in palaeozoic rocks. Examples of tracks simulating Algoidal structures are given which will illustrate the difficulties in distinguishing between true fossils and inorganic markings. We believe Mr Seward is correct in the doubts he holds as to the vegetable nature of *Chondrites verisimilis*, Salter.

It is impossible to refer in detail to the many interesting points touched on by the author in his admirable treatment of this very difficult class of fossils, but *Nematophycus* — the *Prototaxites* of Dawson—deserves a passing notice. This plant is very fully gone into by Mr Seward, who arrives at the conclusion that "on the whole it is probably the better course to speak of *Nematophycus* as a possible ally of the brown Algae rather than as an extinct type of the Siphoneae," and this is going quite as far as our knowledge of the fossils warrants.

In chapter viii. the Bryophyta are considered. We are here treading on very difficult ground, and especially in regard to those species discovered in the older rocks, none of which seem to be altogether free from doubt.

When we reach the Pteridophyta, chapter ix., one possesses more certain data from which to form an opinion of the affinities of the various fossils described, though even here there is room for much difference of opinion.

The Equisetaceae are first described,—a sketch of *Equisetum* prefacing the study of its fossil allies, and here are met with some of the most interesting fossils one requires to consider.

The genus *Equisetum* has been recorded by several writers from Palaeozoic rocks, but Mr Seward places all these in *Equisetites*, and as

there is no absolute certainty that any of these so-called palaeozoic Equiseta are identical with the recent genus, he probably takes the right course. Among those described are *Equisetites Hemingwayi*, Kidston sp.; *Equisetites spatulatus*, Zeiller; *E. columnaris*, Brongt.; and *E. lateralis*, Phill. The consideration of *Phyllotheca* and *Schizoneura* lead us up to the Calamites—which is one of the most important groups—(for it cannot be considered a genus in any true sense of the term)—of palaeozoic times.

The study of the Calamites is introduced by a historical sketch, and this is succeeded by a description of the anatomy of the stems, leaves, roots, and cones, each of which is illustrated by good figures.

This class he treats with great skill and shows a complete mastery of a very difficult subject. He recognises three types of Calamite stems as determined from internal structure:—

Arthropitys, which is the type commonly met with in the Lancashire and Yorkshire Coal Fields.

Arthrodendron, which is very rarely met with in Britain.

Calamodendron, which hitherto has not been recorded from Britain.

The structure of the leaves is next given, after which follows a description of the Calamitic genera that are founded on impressions. These are:—*Calamocladus* (*Asterophyllites*), of which *Calamocladus equisetiformis* is fully described as a typical form. This is succeeded by a description of *Annularia*, *A. stellata* and *A. sphenophylloides* being given in full detail. The roots of Calamites are next dealt with, and with them is now associated the *Astromyelon* of Williamson.

Three types of Calamitic cones are described. These are *Calamostachys* (with which is united *Stachannularia*); *Palaeostachya* and *Macrostachya*. *Huttonia* is also referred to, but with the exception of the first two genera, little is known of their internal structure, without which their true position cannot be satisfactorily determined. Much still requires to be done in the elucidation of Calamitic cones and great care must be exercised in forming opinions as to their affinities.

The impressions and casts of stems are now considered and are classed under the three sub-genera of Weiss:—*Calamitina, Stylocalamites* and *Eucalamites*. The study of Calamite stems presents considerable difficulty. Many specimens which have been regarded as stems are certainly only the casts of the pith cavity, but others, such as many *Calamitina* show the outer surface of the stem. These three sub-genera are distinguished by the manner in which the branches were borne on the stems—a system of classification which can only be regarded as provisional. In very few cases can the foliage and cones be associated with their parent stem.

Archaecalamites, from the Lower Carboniferous and Devonian, forms another group, which is closely related in some of its characters to *Calamites*, but is clearly separated from that genus by several important differences in the stem and cones.

Chapter xi., which concludes the volume, is devoted to the Sphenophyllales.

As in the previous groups the structure of the stem of *Sphenophyllum* is first described. Owing to the numerous specimens of *Sphenophyllum* which have been discovered showing fine structure, the develop-

ment of the vascular bundle from its most rudimentary form to that of the fully developed stem is fully known. These are illustrated in the work before us. The structure of the stems of *Sphenophyllum insigne* and *Sphenophyllum plurifoliatum* are given in detail.

The cones of *Sphenophyllum* offer an interesting field of study. Though the vegetative system of all the species possess many points in common, the sporangia in their number and details of attachment to the bract, differ considerably in the few species of which we possess any concise knowledge. As illustrating these differences in the structure of *Sphenophyllum* cones, several species are described in detail under the generic name of *Sphenophyllostachys*. The first, *Sphenophyllostachys Dawsoni*, Will. sp., is almost certainly the cone of *Sphenophyllum cuneifolium*, Sternb. sp. In this cone each bract bears three sporangia and each sporangium is supported on a slender sporangiophore, but as all the sporangiophores arise from the same point on the bract and are placed in sequence, the sporangium most distant from the axis of the cone has a much longer sporangiophore than that next the axis. In *Sphenophyllostachys Roemeri*, Solms Laubach sp., the incurved end of each sporangiophore bears two sporangia. There is here also, probably, three concentric circles of sporangia. In *Sphenophyllum trichomatosum*, Stur, each bract seems to have borne a single sessile sporangium. It is therefore seen that although in the vegetative system, all the *Sphenophyllum* possess many common characters, in the arrangement of the sporangia, the cones show important differences. Mr Seward rejects, and we believe correctly, the idea that *Sphenophyllum* was an aquatic plant. There is absolutely no evidence in support of this view and very much against it.

Mr Seward's "Fossil Plants" is a most successful treatment of a difficult subject. All of importance is brought forward and impartially discussed and numerous references are given to the original papers consulted. The work however is not a compilation, but embodies the opinions of one who has done much good original work in Palaeophytology. Such a book has long been a desideratum, and its appearance must give a great stimulus to the study of fossil botany in Britain. Mr Seward's style is clear and concise, and the many pitfalls into which beginners are apt to stumble are clearly pointed out. We heartily congratulate the author and publishers on the completion of the first volume of "Fossil Plants," and have only to express the hope that ere long the completing volume will be issued.

A full list of the works referred to in the text is appended and the index, a most important matter, is very full. The illustrations are also good and well chosen. K.

BAU UND LEBEN UNSERER WALDBÄUME. By Dr M. Büsgen. 8vo, pp. viii + 230, with 100 figs. in the text. Fischer, Jena, 1897. Price, 6 marks.

DR BÜSGEN has produced a useful introduction to the study of forestry, which will, doubtless, find a welcome in the various forestry schools on the Continent. Naturally, much of the subject-matter is similar to that of the general text-book of botany, embracing the principles of the anatomy and physiology of plants. But the woody plant is always kept in view, and made to supply the necessary illus-

trations. The result is a book which may take the place of the elementary botanical text-book, and at the same time give the prospective forester some idea of the more special part of his work.

The scope of the volume may be gauged from a review of the headings of the fifteen chapters. Chapter i. deals with the general habit of the tree as determined by its mode of branching, a study eminently fitted for the half of the year during which the trees are leafless. In chapter ii. the causes of the tree-form are discussed, including the direct influence of the external conditions, gravity, light, and wind. The bud is the subject of the third chapter, which ends with a table in which a number of woody plants are arranged in a clavis, according to the position and external appearance of the buds. As might be expected, the grouping by no means follows the natural affinities, unlike plants being thrown together, such as mountain ash, walnut, and like (*e.g.* willow and poplar), often widely separated. The remaining chapters refer to the general structure of stem, leaf, root, and the function of individual tissues, always with special reference to the woody plant; chapter viii., for instance, is devoted to the consideration of the density and structure of various woods. The last chapter deals with the flower and fruit, and the germination of the seed. The illustrations, many of which are borrowed, are considerably below the average of the general text-book.

The Anthropology of Peru

The work of compiling bibliographies is apt to receive far less recognition than it deserves, considering the great value of such work to students, and we are glad to have the opportunity of saying a word in praise of the "Bibliography of the Anthropology of Peru," by Mr G. A. Dorsey, recently published by the Field Columbian Museum, Chicago. This should certainly prove of considerable use, since it appears to have been compiled with considerable care and much labour, and has the appearance of being a very complete list of works written upon this interesting region. That there should be omissions is inevitable, but so firm a foundation will readily bear an appendix. All the known editions of the earlier Spanish works are given, and, what is very welcome, short biographical notices of some of the early writers are appended. The arrangement is by authors, alphabetically, and in a second part, to be published later, an index by subjects and topics is promised, and will add much to the usefulness of the work. We might suggest that an appendix of abbreviated titles, chronologically arranged, would have its value, especially in the case of the contemporary writers. This bibliography was commenced in 1893 in the form of a card catalogue for the compiler's own use in his special studies, but, fortunately, Mr Dorsey was persuaded to continue and complete the work for the benefit of others. That the labour was great cannot be doubted any more than that the result was well worth the pains. "This is my first attempt at bibliography," says Mr Dorsey, "it shall be my last as well. . . ." This remark seems to echo a sigh of relief on the completion of a long and tedious piece of work, but many, to whom such bibliographies are of great value, will regret the latter part of the sentence.

Invertebrata of France

Fauna de France. Thysanoures, Myriopodes, Arachnides, Crustacés, Némathelminthes, Lophostomés, Vers, Mollusques, Polypes, Spongiaires, Protozoaires. By A. Acloque. 1664 illustrations. 18°, pp. 500. Paris: J. B. Baillière et Fils. Price 10 frs.

This is the third volume of Mr Acloque's Fauna of France, Coleoptera appearing in 1896, Orthoptera and the remainder of the insects in 1897. We are promised a fourth and last volume to contain the Vertebrata and Tunicates. We are also indebted to the author for a flora which appeared in 1894. Acloque takes his subject genus by genus, giving a brief diagnosis of each of them, then following on with the species recorded from France, he similarly gives short diagnoses and localities. The numerous figures are sketchy, but no doubt characteristic and useful for identification. There is room for a book on this plan dealing with the English fauna.

Bibliography of Mexican Geology

Bibliografia Geologica y Minera de la Republica Mexicana, formada par Rafael Aguilar y Santillan. 4°, x+158 pp. Mexico: Oficina tipografica de la Secretaria de Fomento. 1898.

Dr Rafael Aguilar deserves hearty thanks for this excellent and compendious Bibliography. No less than 1953 items in the geology of Mexico are sufficient to appal anyone who knows the weakness of our London libraries on special subjects of this kind. The list is arranged in double columns, under authors in alphabetical order, while the last eight pages are devoted to indexes of the principal localities cited and of the more important matters. It is well and clearly printed and will be a great accession to all geological libraries and students.

Bibliography of Westralian Geology

The first Bulletin of the Geological Survey of Western Australia, is devoted to a Bibliography by Mr A. Gibb Maitland. Mr Maitland, who was formerly on the Geological Survey of Queensland, has arranged this under authors, and like so many of these useful works it was originally compiled for his own convenience. Mr H. P. Woodward, his predecessor, assisted Mr Maitland with a list of works extracted from the catalogues of the British Museum. As the Bibliography contains Papers, Reports, and Maps bearing upon the Mineralogy, Mining, Geology and Palaeontology of Western Australia, we need not refer further to its importance.

Dr Chas. Davison hopes to publish, with Messrs Cornish of Birmingham, a volume on the Hereford Earthquake of December 17th, 1896, provided that a sufficient number of subscriptions be obtained to cover the cost of its production.

This earthquake was one of the most important ever recorded in this country. Though inferior to the Essex earthquake of 1884, with regard to the damage done to buildings, its disturbed area was at least twice as great, being not less than 100,000 square miles. It was felt in every English county, except the three northern ones, over the whole of Wales and the Isle of Man, and in the eastern counties of Ireland. The number of observations on which the discussion is

founded is 2902, coming from 1940 different places, and in no previous case have the observations been so detailed and interesting.

Among the facts and conclusions of scientific importance which Dr Davison claims to have established with regard to the Hereford earthquake the following may be mentioned:—The position of the centres of disturbance is determined, and also the direction and hade of the originating fault. It is shown that there were two entirely distinct centres, lying in a north-west and south-east line, and separated by a few miles, the north-west centre being the first in action by a few seconds. A series of new lines called 'isacoustic lines' (or lines of equal sound-audibility), is drawn; these throw an important light on the origin of the earthquake. Coseismal lines (or lines passing through places where the shock was felt at the same instant) are drawn for the first time with an approach to accuracy, and by their means the average velocity of the earth-wave (which, in the case of any but a very strong shock, was unknown) is determined. While the estimates of the direction of the movement in a limited area vary widely among themselves, it is nevertheless found that the average of all these directions passes through the centre.

We hope that the publication of so interesting a work will not be prevented by any backwardness on the part of British geologists.

SCRAPS FROM SERIALS

In the *Transactions of the Manchester Microscopical Society*, there is a paper on Botriomyces a microcosm which produces tumour of the jaw in oxen chiefly, and was formerly regarded as a malignant cancer known as osteo-sarcoma. This is by Mr Worstenholme. Mr Gillanders reviews the Hemiptera-Homoptera, and Mr Mark Sykes treats of Natural Selection in the Lepidoptera. This latter paper, which we hope to notice elsewhere, is beautifully illustrated by eight plates.

The *Proceedings of the Royal Society of Victoria* (vol. x. pt. 2) contains papers by W. S. Dun on some new Upper Silurian Corals; J. Dennant on a new Unio from the River Glenelg (*U. glenelgensis*; a much wrinkled form resembling young examples of *U. australis*); E. R. Waite on Muridae from Central Australia with two new generic names founded on species of Gould's (*Podanomalus* and *Thylacomys*); J. Dennant and Clark on the Miocene of the Gippsland Lakes area; Pritchard and Gatcliff on *Coralliophila wilsoni*, a new gasteropod from Port Philip; Baldwin Spencer on Initiation Ceremonies in the Arunta Tribe; T. S. Hall, Stylasteridae from the Victoria Tertiaries with a new genus *Deontopora*; Officer and Hogg the second part of the Geology of Coimaidai; and Ada M. Lambert on a new land Leech (*Philaemon pungens*; Blanchard, undescribed).

In the *Journal of Conchology* for July there is an interesting account of the pairing of *Limax maximus, L.*, by Mr Lionel E. Adams, fully illustrated by Dr J. W. Taylor. Observations were made on the whole performance and much information concerning the curious suspensory threads was obtained; the anatomy of the parts is also given by Mr W. M. Webb.

THE *Boletim de Museu Paraense*, October 1897, contains papers on the Simii of the New World by Hermann Meerwarth, with maps of the distribution of each genus, further notes on the geology of Brazil by Fred. Hartt, the Devonian fauna of the river Maecuru by Friedrich Katzer, notes by Dr Goldi on *Lepidosiren* and *Mesomys ecaudatus*, and J. Huber on plants of the genus *Hevea*. All the papers are in Portuguese.

THE *Memorias Sociedad "Antonio Alzate"* (vol. xi., Nos. 5-8) contain the education of the Mexican woman by Galindo y Villa, the origin of individuals by Professor Herrera, public instruction in Mexico by Torres Torija, gold in Mexico by E. Ordonez, the water supply of the city of Mexico by Dr A. Peñafiel, and seismic notes from Central and South America by Montessus de Ballore. Herrera's, Ordonez', and Montessus' papers are in French, the others in Spanish.

THE first part of Dr Arthur Willey's "Zoological Results based on material from New Britain . . . and elsewhere," contains the anatomy and development of *Peripatus novae-britanniae* by the editor, *Metaprotella sandalensis* a new Caprellid by Dr Paul Mayer, *Aipysurus annulatus* a rare marine snake by G. A. Boulenger, reports on the centipedes, millipedes, scorpions, pedipalpi and spiders by R. I. Pocock, and an account of the Phasmidae and their eggs by Dr D. Sharp.

The Geological Survey of India notify that by order of the Government the "Records" issued by their department in the months of February, May, August and November each year, ceased to be published from the 1st January 1898. Annual Reports will be issued, and papers will appear in the Memoirs.

FURTHER LITERATURE RECEIVED

ZOOLOGICAL results based on material from New Britain, New Guinea, Loyalty Islands, and elsewhere, collected during 1895 7, Arthur Willey: Cambridge Univ. Press. Composition of Maize, W. H. Wiley: U.S. Dept. Agric. Bull. 50. Geology for Beginners, W. W. Watts: Macmillan. Faune de France (Thysanoures—Protozoaires), Acloque: Baillière et Fils. Bibliografia geologica y minera de la Republica Mexicana, Rafael Aguilar: Secretaria de Fomento, Mexico. Report on West Australian Rocks, G. W. Card. Darwin Wrong, R. F. Licorish: Barbados. Studien über die Protoplasmaströmung bei den Characeen, by Georg Hoermann: Gustav Fischer. Wisconsin Geol. and Nat Hist. Survey, Bulletins i., ii. Manual of the Geology of India, Economics, ed. 2, part i., Corundum by T. H. Holland: Geol. Survey, Calcutta. History of Mankind (Macmillan), pt. 28. *Mem. Soc. Antonio Alzate*, xi., pts. 5-8. *Proc. Biol. Soc. Washington* (numerous papers by Bangs and Preble). College of Agriculture and Mechanical Arts, Mesilla Park, Annual Catalogue. Annals of the South African Museum, part 1: Trustees. On Pontobolbos, and other papers, Arthur Dendy. Additions to the fossil Flora of Queensland, J. Shirley.

Amer. Geol., Aug.; Amer. Journ. Sci., Sept.; Amer. Monthly Micro. Journ., Aug.; Amer. Nat., Aug.; Botan. Gazette, Aug.; Feuille des jeunes Nat., Sept.; Irish. Nat., Sept.; Knowledge, Sept. 1; Literary Digest, Aug. 13, 20, 27; Naturae Novit., Aug., Nos. 15 and 16; Naturalist, Sept.; Nature, Aug. 18, 25, Sept. 1, 8; Nature Notes, Sept.; Naturen, June-Aug.; Photogram, Sept.; Plant World, Aug.; Psychol. Rev., Sept.; Review of Reviews, Aug., Sept.; Revista Quin. Psich., Aug.; Revue Scient., Aug. 20, 27, Sept. 3, 10; Science, Aug. 19, 26, Sept. 2; Scientific Amer., Aug. 13, 20, 27, Sept. 3; Scot. Med. and Surg. Journ., Aug., Sept.; Scot. Geogr. Mag., Sept.; Victorian Nat., Aug.; Westminster Rev., Sept.

OBITUARIES

NICOLAS AUGUSTE POMEL

Born 1821. Died August 1898

The death of Pomel removes from Algeria a distinguished mineralogist and geologist and one who had made for many years a particular and detailed study of the country and its palaeontology. Auguste Pomel commenced his geological career by writing in 1842 several papers on the geology of the Auvergne; his thirty-fourth paper (1854) was the first written on African matters, since when he has published little short of 100 papers on Northern Africa. Perhaps his best known work is his essay on the classification of the Echinoids in which he founded new genera by the score. The work was regarded with such disapproval that it was deliberately ignored by the Zoological Record, which declined to record his swarm of new generic terms; and it was denounced with much vigour by the late Professor Duncan and Mr Sladen, in a paper for which the title of "Pomelism and Crime" was suggested. His "Catalogue méthodique du mammifères tertiaires," 1853, is a wonderful book, and was undeservedly discredited by Paul Gervais and other writers.

JOSEPH CHARLES HIPPOLYTE CROSSE

Born 1827. Died 7th August 1898

We regret to record the death of this distinguished conchologist, which occurred at Vernou (Seine-et-Marne) at the age of 71 years. Mr Crosse was the editor of the *Journal de Conchyliologie*, a journal founded by Petit de la Saussaye, and continued by Paul Fischer and Bernardi until 1861, when Crosse took the place of Bernardi, and the two raised the *Journal* to one of first importance. Among Mr Crosse's chief works we may mention the Mollusca of Mexico and Guatemala in the Mission Scientifique au Mexique, and the Mollusca in Grandidier's Madagascar. He worked chiefly on exotic forms and contributed frequently to his own Journal.

FÉLIX BERNARD

Born 1863. Died August 1898

Another zoologist of great promise has been removed at an early age in the person of Félix Bernard, of the Paris Museum. He was best known by the series of papers on the hinge of the bivalved mollusca, which considerably advanced the study of that difficult group, and which have been noticed at length in these pages. He also wrote "Eléments de Paléontologie," 1895.

MR G. E. GRIMES, whose appointment to the Geological Survey of India we announced in October 1895, succumbed to an attack of cholera at Thayetmyo, Burma, on the 11th April last. Mr Grimes had shown great promise as a stratigrapher during the two and a half years he had been in the service, and was only twenty-six when he died.

JOHAN LANGE, the distinguished botanist, died at Copenhagen, April 3, aged eighty. His principal works were a Manual of the Danish Flora and the last ten volumes of the Flora Danica. He was President of the Danish Botanical Society for twenty-seven years; for twenty years Director of the Copenhagen Gardens; and was also Professor of Botany at the Agricultural College there.

Among others whose deaths have been recently recorded are :—FREDRICH CHARLES APLIN, the ornithologist, at Bodicote, aged 43 ; Dr E. B. AVELING, formerly assistant in physiology at Cambridge and professor of chemistry and physiology at New College, well known as a populariser of evolution and a lecturer and writer on socialism, died in London on August 4, aged 47 years ; EVERT JULIUS BONSDORFF, formerly professor of anatomy and physiology at Helsingfors University, aged 88 ; LUIGI BALZAN, the arachnologist of the University of the Ascension, Paraguay ; AXEL GUTTBRAND BLYTT, professor of botany at Christiana University, on July 25, aged 54 ; Dr ERNEST CANDÈZ, the coleopterologist at Glain, near Lüttich, on June 30 ; PASQUALE CONTI, the botanist, who died at Largano after a lingering illness ; Dr DEVRY, the naturalist and physiologist, best known for his researches into the pharmaceutical properties of quinine, at the Hague, on August 7 ; The Right Hon. MURRAY EDWARD GORDON FINCH-HATTON, twelfth Earl of Winchelsea, well known for his agricultural interests, on September 7, aged 48; Dr D. P. FRAME, veterinary surgeon and microscopist, at Kansas City, on February 25 ; CARLO GIACOMINI, professor of anatomy at Turin University on July 5 ; C. W. A. HERMANN, the mineralogist, at New York on June 21, aged 97 ; Professor D. S. KELLICOTT, one of the best known microscopists in America, who died at the Ohio State University in April last; JOÃO MARIA MONIZ, the botanist, on July 11 at Funchal, aged 75 ; the metallurgist, BERNARD MÖBIUS, while travelling from America to Europe on May 17, aged 46 ; Professor PARK MORRILL, of the Forecast division of the Weather Bureau of the United States, died at Washington on August 8 of typhoid fever ; HENRI VANDER MEUTEN, a well-known horticulturist, at Ixelles, Belgium, on July 24, aged 83; CARMELO SCINTO PATTI, geologist and engineer of Sicily, born January 21, 1829, died February 7, 1898 ; ACHILLE POITAN, an enthusiastic naturalist of Aubervilliers and the canton of Pantin, aged 23 ; Dr E. LEWIS STURTEVANT, the agronomist, on July 30 at Framingham, Mass., aged 56 ; W. F. R. SURINGAR, professor of botany in the University of Leyden, and Director of the Gardens and Herbarium there ; Major-General ROBERT GOSSET WOODTHORPE, closely associated with the geographical exploration of India, born 1844, died May 26, 1898, of whom a long obituary notice and a portrait appears in the *Journal of the Royal Geographical Society*.

NEWS

THE following appointments have recently been made :—Leopold Adametz as veterinary professor at the High School for Agronomy at Vienna ; Prof. W. P. Blake, of Tucsan, Arizona, as State Geologist of Arizona ; Dr Carl Brick as assistant in the Hamburg Botanic Garden ; Rudolf Beyer as honorary professor of botany in Berlin ; Friedrich Blochmann, of Rostock, as professor of zoology at Tübingen University ; Friedrich Moritz Brauer as director of the zoological collections in the Hof-Museum, Vienna ; Dr W. T. Brigham as director of the Berenice Panahi Bishop Museum at Honolulu ; Dr Frederick E. Clements as reader in botany in the University of Nebraska ; Hermann Dingler as professor of botany at the Institute of Forestry at Aschaffenburg ; Dr O. V. Darbishire as demonstrator in Botany at Owens College, Manchester ; Benj. M. Duggar and Dr Elias I. Durand as readers in botany at Cornell University ; Dr Adrian Fiori as private docent in botany, Padua University ; Albert Heischmann, of Erlangen, as professor of zoology at the University there, in the room of Dr Selenka ; Dr Karl Fritsch as director of the botanical museum at the University of Vienna, in the room of the late Kerner von Marilaun ; G. T. Hastings as assistant in botany at Cornell University ; Dr Franz Hoffmann as private docent for physiology in the University of Heidelberg ; Dr J. Jablonowski, of Berlin, as assistant in anthropology at the Dresden Museum ; Ludwig Katharina as professor of zoology and comparative anatomy in Freiburg University ; George Klebs, of Basle, as professor of botany at Halle University ; Dr Hans Lenk as professor of mineralogy and geology at Erlangen University ; Ludwig Kerschner as professor of histology at the University of Innsbrück ; H. J. Monson and Andrew Linton as senior and junior professors of agriculture to the School of Agriculture at Ghizeh ; W. A. Murrill as assistant in botany at Cornell University ; Lubomir Niederle as professor of archaeology and ethnography in the Bohemian University of Prague ; Dr C. C. O'Hara as professor of geology and mineralogy in the South Dakota School of Mines ; Dr Ph. Pocta as professor of palaeontology at the Ceske Museum, Prague ; Mr Gifford Pinchot, as chief of the Division of Forestry in the U.S. Department of Agriculture, in the room of Dr B. E. Fernow, who becomes the head of the New York School of Forestry ; Dr Aladar Richter as chief of the botanical department of the Hungarian National Museum, and Paul Kuckuck, of Heligoland, to be keeper of botany there ; Dr Ludwig Reh as zoological assistant at the Concilium Bibliographicum, Zürich ; Dr F. J. V. Skiff, of the Field Columbian Museum, as director of mining and mineralogy at the Paris Exposition of 1900 ; C. F. Myers-Ward as lecturer in physiology at the Owens College, Manchester ; Dr A. Weberbauer as private docent for botany in Breslau University ; Dr Zograf, extraordinary professor of zoology, and Dr Mrensbier, extraordinary professor of comparative anatomy in Moscow University.

LORD PEEL has been appointed a trustee of the British Museum in the place of the late Mr Spencer Walpole.

DR C. H. HITCHCOCK of Dartmouth, U.S., has left for a year's geological exploration in the Hawaiian Islands. His address will be Honolulu.

WE regret that we entirely overlooked the fact that the Government made a substantial grant from the Royal Bounty of £150 to Mr Joseph Wright of Belfast

for his long and valuable researches into the palaeontology of the rocks of Ireland. We were quite aware that this had been a possibility for some years past, and hasten to congratulate Mr Wright on his well-deserved distinction.

THE monument to Charcot will be formally unveiled on October 23rd, in the Salpêtrière, Paris.

A LIFE of William Turner of Cambridge, 1507-1568, one of, if not the earliest British zoologist, has been contributed to the *Zoologist* for August, by the Rev. H. N. Macpherson.

A MEMOIR of Fritz Müller, the Brazilian naturalist, is to be undertaken by Dr A. Möller, of Eberswalde. Dr Möller begs the loan of letters or material that will help him in his task.

THE following grants have been made by the Berlin Academy :—2000 marks to Prof. Engler, for East African plants; 600 marks to Prof. Graebner, for the study of German Heaths; 500 marks to Dr Loesner, to complete his monograph on the Aquifoliaceae.

MR A. J. HERBERTSON, lecturer on geography in the Heriot-Watt College, Edinburgh, has obtained the degree of Ph.D. *multa cum laude* in geography at the University of Frieburg, in Baden. Dr Herbertson's thesis was on the "Distribution of rainfall over the earth's surface," a subject which he has investigated while compiling the rainfall maps for the physical atlas about to be published by Bartholomew.

THE Hon. John Macgregor has presented a cheque for £500 to the fund for the endowment of a chair of Forestry in the University of Edinburgh. It will be remembered that the Royal Scottish Arboricultural Society has asked the Government for a grant for the establishment of a State Forest near Edinburgh for research in forestry.

THE Swiney Lectures on Geology, under the direction of the Trustees of the British Museum, will be delivered by Dr R. H. Traquair on Mondays, Wednesdays, and Fridays at 5 P.M., beginning Monday, October 3. They will be on the Palaeontology of Great Britain, and will be given in the Lecture Theatre of the South Kensington Museum.

THE New Whale Gallery at the British Museum is the subject of an illustrated article by Mr Lydekker in *Knowledge* for September 1. Owing to the difficulty of position, however, the photograph does not give one a proper idea of the gallery, which is well worthy a visit even from those not specially interested in zoology.

WE learn from *Science* that the Lacoe collection of fossil insects contains the types of about two-thirds of those described from North America. Besides these there are 3500 specimens from the Oeningen Tertiaries, and a large collection from Florrisant, Colorado. The United States National Museum has now perhaps a collection of fossil insects second to none, in any case it has a collection of the first importance.

THE late Professor Victor Lemoine bequeathed his palaeontological collection to the Paris Museum. In order that the collection may be further supplemented Madame Lemoine has handed over the land at Cornay, near Rheims, whence the fossils were obtained, to the same institution.

THE South African Museum has so far advanced as to issue "Annals of the South African Museum," a handsome octavo serial, well illustrated by lithographic plates, and printed and published in London by West, Newman & Co. It will appear at irregular intervals, as matter for publication is available, and will

be devoted to the researches of the museum staff. The first part, which is dated June 1898, contains papers on Scorpions by Purcell, Mutillidae by Péringuey, Reptiles by Sclater, and Hispinae by Péringuey. The Trustees report that the whole work of transferring the collections from the Old Museum to the New Museum Building was accomplished in a month, with practically no damage or loss, at a cost of approximately £90.

The American Museum of Natural History, Central Park, New York, is rapidly progressing with its new buildings. These consist of a corner to the west wing and a lecture hall, the latter of which may be ready for occupancy this year, and the former ready for cases and fittings during 1899. Among the excellent work done by this Museum is the fitting out of expeditions for special objects: thus Dr Carl Lumholtz has returned after four years spent among the tribes in Mexico with a large and valuable collection; Dr Adolf Bandalier has continued his researches in Bolivia and Peru; Mr Ernest Volk has been employed for the whole year exploring near Trenton, N.J., for the purpose of careful investigation of the question which has arisen relative to the antiquity of man in the Delaware Valley; while Mr A. J. Stone begins this year, and will continue till 1900, collecting vertebrate Zoology from Montana to Bering Strait. Two great dinosaurs in a remarkable state of preservation have been secured from Wyoming, and a complete skeleton of the three-toed horse has also been obtained for the collection. The Library grows rapidly, and many scarce works on Zoology have been added to the shelves, while the Duke of Loubat has been a generous donor in the department of Mexican and other ethnology.

The National Herbarium of the United States has received from Dr W. H. Forwood his collection of plants of Western Wyoming, collected in 1881 and 1882. *The Plant World* states that the collection forms the basis of two scarce reports published by the War Department. Many of them are also referred to in Tweedy's Flora of the Yellowstone.

The Report of the Keeper of the Manchester Museum refers to the installation of electric light, which has been rendered possible by the generosity of Mr Reuben Spencer, who contributed £500 to the expense. The Museum is at present in the hands of the painters, and it is to be hoped that the committee will sanction the general whitening of the ceilings asked for by Mr Hoyle, in order that the electric light may have a good start. Prof. Hickson has been doing good work on the plankton of Lake Bassenthwaite, and some of the rarer forms will shortly be placed on exhibition. Miss Nördlinger, the keeper's efficient secretary, receives due eulogies, and we are glad to hear that she has taken entire charge of the library and hope she will be able to open the proper purse-strings for much needed additions. The committee have undertaken the printing of Mr Sherborn's index to the 10th and 12th editions of the "Systema Naturae" of Linnaeus, which should prove of value to zoologists, as these books form the starting-point of zoological nomenclature. A series of lectures will be delivered by Prof. Boyd Dawkins on certain Saturdays and Sundays between October and June, other lectures to be delivered by the staff as usual. Mr Hoyle closes his Report with an eloquent appeal for more funds, and it really does seem singular that Manchester can only afford an expenditure of £2785 a year on its Museum, while Liverpool spends £5700. Manchester must wake up.

A useful part of Mr Hoyle's Report referred to above is his account of the twenty-five museums visited by him while travelling in the United States and Canada in 1897; the list, however, is too long to quote here.

The Keswick Museum, which was founded in 1873 in connection with the Keswick Literary and Scientific Society, was removed early this year to Fitz Park,

Keswick, and is now known as the Fitz Park Museum. It owes its origin to the late James Clifton Ward, whose valuable geological work in the lake district is well known. The present curator is Mr James Postlethwaite. The collections are restricted entirely to objects illustrative of the local natural history, and although some of the sections are still far from complete, considerable energy is being displayed to make it exhaustive. A catalogue was issued in 1888, and this, we hope, will soon be followed by a new edition.

SOME 3000 members attended the meeting of the British Association at Bristol, September 7-14, under the presidency of Prof. Sir William Crookes. Lectures were delivered in the evenings by Prof. Sollas on "Funafuti, the study of a Coral Island," and by Mr Herbert Jackson on "Phosphorescence," while Prof. Poulton delivered the Working Man's Lecture on "The ways in which animals warn their enemies and signal to their friends." A special biological exhibit was arranged in the Zoological Gardens, consisting of living hybrid trout, specimens of cross-breeding in animals, and hybrid and crossed varieties of flowers, ferns, orchids, and other plants. The First Lord of the Admiralty stationed, by request, four Battleships in Kingroad, Avonmouth, for the edification and protection of the visitors. An excellent series of excursions took place, those most interesting to our readers being Austcliff on Sept. 10 to see the Rhaetic beds, and to Tortworth on Sept. 15 to see the new exposure of Silurian beds recently re-opened by Lord Ducie. This proves to be a thin band of Wenlock bordering the exposure of Upper Llandovery, and is crowded with *Coenites*. A long excursion of five days was taken from Sept. 16-20 to Exeter, Torquay, Dartmouth, Plymouth, and Dartmoor. In a comprehensive pocket handbook which was issued, Prof. Morgan gave a sketch of the geology of the district, Mr J. W. White of the botany, Mr A. E. Hudd of the insects, and Messrs Morgan and Charbonnier of the vertebrata, with the exception of the birds which were dealt with by Mr H. C. Playne.

THE Royal Society of Victoria has had a shock not uncommon to societies in the Australian continent, viz., the reduction of its Government grant. We echo the hope of the Council in their last report that "with a return of more prosperous times the vote may be increased so as to enable the Society to publish the papers presented to it." There is a steady growth of the library as indicated by an additional 200 feet of shelving erected during the year.

THE Manchester Microscopical Society has issued a satisfactory report for 1897. There is a loss of two members, but that no doubt will be regained next year. The library and the collection of slides are both increasing, and the latter is carefully listed out at the end of the current transactions.

THE Edinburgh meeting of the British Medical Association was commemorated by the *Scottish Medical and Surgical Journal* in a special number, issued as volume iii., No. 2, for August, price two shillings. This is well illustrated with photographs of the Presidents, University old and new buildings, M'Ewan Hall, Royal Infirmary, and many Scottish Spas. Among other interesting articles are Medical Institutions in Edinburgh, Medical Student Life in Edinburgh, Edinburgh Medical Clubs, their Songs and Song-Writers, the Edinburgh Royal Infirmary Old Residents' Club, and a general account of Scottish Spas and their mineral waters. A photograph of the Residency table at the Royal Infirmary, covered with names of past residents, will awaken many memories.

THE roll of the Field Naturalists' Club of Victoria is 129 members, a slight decrease on that of last year. Its journal, the *Victorian Naturalist*, has commenced its sixteenth year and is edited by Mr F. G. A. Barnard. One of the chief works of the year has been the protection of the albatrosses on Albatross

Island, the result of a deputation to the Premier of Tasmania. They are now safe all the year round for five years. Mr C. French was again elected president.

THE Council of the Royal Geological Society of Cornwall in its 84th Report expresses its satisfaction over the new geological survey of the county at the hands of Mr J. B. Hill. Application to the Government was made as the result of the Annual Joint Meeting of the Scientific Societies of Cornwall, at Falmouth, in August 1896, and Mr Hill was told off by the Survey to examine the sections of the south coast last autumn. The Council also reports the complete and detailed examination of the St Erth pliocene, and has under its consideration the preservation of the plans and sections of abandoned mines. Mr Howard Fox has been awarded the Bolitho gold medal, and Mr J. H. Collins has been made an honorary member.

FROM the Annual Report of the Yorkshire Geological and Polytechnic Society we learn that the roll of members is 164. This is the highest since 1893, and it is satisfactory to learn that all these members are in active association with their Society. The editors have dated their *Proceedings* with the proper year of issue, instead of one year earlier as heretofore. Next year we hope they will improve on this and add the month, for we note that the future bibliographer will not be able to say whether Mr Woodward's paper on the Yorkshire fossil fishes was published in January or December 1898. The *Proceedings* contain a paper on Filey Bay and Brigg by Mr Fox-Strangeways, which is illustrated by eight beautiful photographs by Mr Godfrey Bingley. There are also portraits and obituaries of Thomas Tate and John Stanley Tute.

THE Selborne Society in the September number of *Nature Notes* desire to wipe off a printer's debt. The Society is now sufficiently flourishing to show but a small deficit in its balance sheet, but hopes to raise three hundred pounds during the next three years to clear itself of debt.

IN June last we called attention to an application for subscriptions to erect a suitable monument to the late Baron Ferdinand von Mueller. This was set on foot by the executors. We now note that a second committee has been formed by Mr W. Wiesbaden, Professor Baldwin Spencer, and others, who are desirous of founding some National Memorial which shall worthily perpetuate his name. Whilst nominally the Government Botanist of Victoria, it is well known that the Baron von Mueller's assistance was sought by and always freely given not only to public bodies but to private individuals in all parts of Australia. Apart from his purely scientific work, upon the value of which it is unnecessary to dwell, Von Mueller devoted himself to the development of the more practical side of various branches of work, such as those connected with Forestry, Agriculture, Horticulture, Pharmacy and, not least, Geographical Exploration. His own explorations in early days, both in Northern Australia as botanist in the expedition under Mr A. C. Gregory, and when, subsequently, he traversed alone the then little known wilds of Gippsland, were of considerable importance, and his deep interest in and the practical assistance which he rendered to the explorations of others are well known. Not only did he spend his whole life in the furtherance of the work in which, from the nature of his position, he was most deeply interested, but he devoted practically the whole of his income to the assistance of those who were engaged in work the object of which was to increase our knowledge of the nature and products of Australasian lands. It is on these grounds, therefore, that the committee hope that sufficient funds will be forthcoming to provide for (1) the erection of some form of statue, and (2) the endowment of a Medal, Prize or Scholarship, to be associated with Von Mueller's name and to be awarded from time to time in recognition of distinguished work in the special branches in which he was most deeply interested, and which

shall be open to workers throughout the Australasian colonies. Subscriptions to the Fund may be sent to the Hon. Treasurer, addressed to the College of Pharmacy, Swanston Street, Melbourne.

THE Report of the Botanic Gardens and Domains of New South Wales for 1897, by Mr J. H. Maiden, has recently appeared, and contains full accounts of the Botanic Gardens, Government Domains, Garden Palace Grounds, Centennial Park, State Nursery at Campbelltown, &c. Mr Hugh Dixson has placed his collection of Australasian orchids at the disposal of the Botanic Gardens, and suitable accommodation is to be speedily provided for their reception. Parliament has also voted a sum of money for the erection of a building to house the Herbarium; the Library shows a steady progress. Altogether a very favourable and hopeful report, and the first of a new series, which is to be continued annually. The last report appeared in 1878.

WE learn from the *Echo* that 100 tuns of beer and 18,000 cups of coffee were consumed at the Berlin Zoological Gardens on Whitsun Day. We are not responsible for the statement, but, if true, it shows that zoology as an interest is not likely to die out in Berlin.

"NATURE" for August 25 has an interesting article on "The Marine Fauna in Lake Tanganyika and the advisability of further exploration in the great African lakes," by J. E. S. Moore. Mr Moore prints a list of empty shells and fishes previously known and also a list of the entire mollusca and fishes obtained during his expedition.

THE Swiss Society Rambertia has laid out an Alpine Garden at Montreux, at an elevation of 6000 feet, where the characteristic trees and flowers of the country are to be cultivated.

AT the moment of going to press we learn that Dr Florentino Ameghino has made a remarkable discovery. Details of a nocturnal quadruped have been brought to him from time to time by Indians, and a few years ago the late Ramon Lista actually saw and shot at a mysterious creature in the interior of Santa Cruz. Apparently bullet-proof, it disappeared into the brushwood, and all search for it proved futile. Lista described the creature as a pangolin, without scales, and covered with reddish hair. Despite the fact that Lista was known to be a good observer, Dr Ameghino could not help feeling that he was deceived. Lista, however, has now been proved correct, for Ameghino received recently from South Patagonia some fresh bony ossicles and a partially destroyed skin. The ossicles were comparable to those of *Mylodon*, but smaller, and they were embedded in the skin, like "paving stones in a street." The skin itself is two cm. thick, and of such toughness that it could only be cut with a hatchet. The surface of the skin itself shows an epidermis, not scaly at all, but covered with coarse hair, four to five cm. in length. and of a reddish grey shade. This Ameghino considers was the animal described by Lista, and as that naturalist has unfortunately lost his life while exploring Pilcomayo, and was the only civilised man who had seen it in the flesh, he names it *Neomylodon listai*. The importance of the discovery need not be emphasised here.

NOTICE

TO CONTRIBUTORS.—All Communications to be addressed to the EDITOR of NATURAL SCIENCE, at 29 and 30 Bedford Street, London, W.C. Correspondence and Notes intended for any particular month should be sent in not later than the 10th of the preceding month.